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PLATE I.

MAURICE LOEWY
1833–1907

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MAURICE LOEWY.

French astronomy having already suffered great losses has just met with one more, one of the most irreparable that could occur. Monsieur Loewy, director of the Paris Observatory, fell a victim to a sudden illness from which he did not recover.

He died at the height of his activity, in full possession of his powers, without his arduous zeal ever having slackened. It is, no doubt, the death he would have wished, one which allowed him to work up to the last minute; for enforced repose would have been for that indefatigable worker a dreadful punishment.

Maurice Loewy was born at Vienna, April 15, 1833; it is there that he studied, that he began to observe, that he had his first works printed. His first publications have for their subject the calculation of the orbits of comets and of small planets; he calculated the ephemerides of several comets, among others of the celebrated comet of Donati. The work in this line to which he devoted the most time is the theory of the planet Eugenia to which he returned several times and for which he finally obtained very exact elements by using three normal places corresponding to three consecutive oppositions and by taking account of the perturbations.

These first researches had attracted the attention of Le Verrier; in Austria at this time the Jews were not treated on an equal footing, and Loewy feared lest his career be trammelled on that account. Le Verrier made offers to him and he accepted them. He entered the Observatory of Paris August 15, 1860, and one year later he was appointed associate astronomer. Le Verrier knew how to discover young talents, to encourage them and to attract them; perhaps having once attracted them he did not continue his encouragement with enough perseverance.

In France Loewy continued his researches in the orbits and introduced improvements in the method of Olbers so as to make it more easily applicable when the three observations are not very near together. In collaboration with Tisserand he also calculated the orbit of the planet Dike, which had been lost for four years. But the field of his activity was quickly

enlarged, and he turned his attention to the detailed study of the instruments. It was he who was appointed to draw up for the *Annales de l'Observatoire* instructions about the use of the equatorials; one finds in this statement the same care for exactness and precision which was to distinguish him later.

He had been serving France for nine years when he was admitted to the great naturalization of 1869. Our country has always known how to attach to herself her adopted sons, who were not the least devoted of her children. Scarcely a year later Paris was invaded by the enemy, and Loewy had to defend his new country upon the ramparts of the capital. The dangers braved for France finished the work of making him a Frenchman. Since then he has liberally paid his debt to France in giving to her forty-seven years of persistent labor and in giving her also valiant sons who, following the example of his life, should become useful workers and good citizens. His writings and especially his work of observation have won for him the esteem of all the scholars, and they proved it to him by making him a member of the Academy of Sciences in 1873. The preceding year he had been appointed a member of the Bureau of Longitudes. These new functions would cause him much work; but an increase of work had never frightened him. For fifteen years with Admiral Mouchez, he was busy with the direction of the observatory of the Bureau of Longitudes, which was installed in the Park Montsouris for the astronomical instruction of sailors and explorers. During thirty years he directed the computers of the Bureau of Longitudes, a work which demands incessant verifications and constant supervisions. He took the interests of these modest co-laborers to heart and defended them valiantly, sometimes at the risk of getting into a quarrel with the managers of the administration who no less legitimately, were preoccupied in managing our finances.

It was thus that he edited more than thirty volumes of the *Connaissance des Temps*; he wanted this ephemeris to remain superior to all similar publications and he continually introduced improvements in it; planned far many more and he was restrained only by the narrow limits of the meagre budget of the Bureau, importunate hindrances which he endured with impatience. And that was not all; it was he who took charge of the astronomical part of the *Annuaire du Bureau des Longitudes* and who unceasingly revised

it; it was he who edited the ephemerides of the stars of lunar culmination which this Bureau published every year as long as its financial resources permitted and finally it was he who almost entirely filled the first volume of the *Annales* with original articles. We see in how many different ways and with what perseverance he co-operated with the learned body which had just called him into its circle.

In 1871 Loewy proposed a new form of equatorial which he was to realize later and from which he was to derive great advantage. It is well-known how laborious is the use of the ordinary equatorial, what continual gymnastics it demands of astronomers. Without doubt their devotion to science is great, and they would not mind the fatigue of it were it not to detract from the value of their observations; but it is clear that this is not the case and that a tired observer will do less satisfactory work. It was these considerations which determined Loewy to invent the equatorial coudé. By means of two reflections by plane mirrors, a luminous ray starting from any point whatever in the heavens he turned into a fixed direction, that of the polar axis. Comfortably seated in an arm-chair and without moving, the astronomer can successively bring into his field of vision any star whatever by operating two little hand-levers placed within his reach. He can even warm himself, which for an astronomer is an unheard-of comfort. To these advantages are added those which result from the great focal distance.

There are also some drawbacks; one may fear lest the plane mirrors cause a loss of light, lest they get out of shape on account of the deflection or the temperature; lest in this double tube, unequally heated, there be produced currents of air or undulations. In fact, at certain times the images may appear a little wavy; but by taking certain precautions learned from experience, astronomers have succeeded in decreasing these disadvantages, and in practice they have proved to be less serious than was feared at first. Two instruments of this kind have been set up in the gardens of the Observatory; it is one of these which was used for the photograph of the Moon with the results already known. The bent equatorial also lends itself readily to the adaptation of the spectroscopes which when they are somewhat heavy it is often difficult to arrange suitably in the awkward position at the end of a long tube.

Monsieur Loewy did not confine himself to having the in-

strument constructed, he established the theory of it and determined, in collaboration with M. Puiseux, the effects of flexure on this complicated system.

The flexure in the meridian instruments also attracted his attention; it exerts a perceptible influence in the measures of declination, and its effects are not to be overlooked even in the measures of right ascension if one wishes to carry precision to its extreme limits; for one can never be assured that the symmetry of the apparatus is perfect and that there is no lateral flexure. Hence the necessity of a direct determination of these flexures. But this determination was difficult; Loewy devised an ingenious apparatus for this purpose and, with the collaboration of Périgand, he applied it to the Bischoffsheim meridian circle. A glass disk cut on four faces is placed inside the tube at the intersection of the optical axis and the axis of pivots; this makes it possible to bring upon the reticule three images at will: the reflected image of the threads of the reticule, that of the marks written on the objective and that of the divisions of a plate adjusted in the axis of the pivots.

The comparison of these numerous images permits of many verifications which are a valuable proof of exactness; it also gives the longitudinal and lateral flexures in the different positions of the telescope. It gives something more still; in the theory of the meridian telescope it is supposed that the pivots are perfect cylinders of revolution, and in fact the makers accomplished this with great exactness. But in sciences of observation the postulates should be submitted to constant revision, for they cease to be acceptable in proportion as people become more exacting and demand more precision. It became necessary to determine the exact form of the pivots. The apparatus of Loewy furnished the means for it several years before M. Hamy had given the splendid solution of this problem which is now known.

I will add to these works those that he did more recently on this same circle in collaboration with M. Renan. Inexplicable anomalies had been noted; the readings made on the right circle and on the left gave different results and the differences had a systematic character. Studies were undertaken to discover the cause; the differences were attributed successively to a flexure of the radii of the circles, and to a torsion of the axis; but these different explanations had to be given up and the real cause was finally found in the manner in which the lines of the division were illuminated; then this

fact, which might have passed unnoticed for a long time, could be remedied. Loewy did not neglect the slightest detail in his study of the instruments; he knew how small a thing it takes to alter their readings.

I now come to a very ingenious method which may be used to determine on the one hand the constant of refraction, on the other that of aberration. These two phenomena both have the effect of varying the apparent angular distance between two fixed stars. The problem then in both cases is to measure the small variations of this angular distance.

But until recent times this measure had not been made directly and one was limited to determining the positions of the two stars to be compared by using meridian observations. The result was, for instance, that one must take account of the uncertainty in the nutation, and in general of every error in the position of the equator and of the ecliptic which serve as planes of reference. One was deprived also of all the advantages of differential processes. Unfortunately, it seemed that these processes could not he applied, since the two stars whose distance was in question were always so far separated the one from the other. Loewy conceived the idea of bringing the two images nearer together by having them reflected upon the two faces of a prism placed before the objective. What was most remarkable was, that errors which might have arisen from a lot of different causes eliminated themselves of their own accord. There was no need of knowing the angle of the prism provided it remained constant; the variations of temperature acted only upon the linear dimensions without affecting this angle; small changes in the orientation of the prism introduced only errors that might be considered as infinitely small of the second order. These are the advantages common to all the differential methods.

Why has this method not yet given all the results that were expected of it? I can not tell; the success was at first encouraging; the difficulties at first experienced in obtaining good images had been surmounted. It seems that Loewy let himself be turned aside from his researches by other ideas which soon demanded all his attention and occupied all his time; it is to be regretted that he did not intrust its continuation to some one of his co-laborers. I believe that we may yet expect much from it.

The principal difficulty in the meridian observations is the determination of the instrumental constants; the position of

the instrumental equator must be compared with that of the real equator. This comparison is made by the observation of stars near the pole. But in the classic method one was content with observing the two passages of the same circumpolar star at a twelve hours interval. The inconveniences of this method of procedure are evident, since the stars observable in the daytime are rare, and since, in a period of twelve hours, the position of the instrument may have varied as well as the rate of the clock. Loewy sought a means of avoiding these difficulties by directing his telescope toward the pole and by determining at each instant, by the aid of a micrometer, the coördinates of different polar stars. Thus one may see in a single night a hundred stars pass by, all included between the second and tenth magnitudes, on which one can effect a series of pointings without waiting for their passage across the meridian. By properly grouping these pointings, by judiciously choosing the time, by re-dividing the stars into couples or groups of four, one can eliminate the causes of systematic error, and can get rid, for instance, of those which might come from the rate of the clock or from the shifting of the instrument during the time of observations. Loewy made numerous notes of this discussion; he left no point obscure, and he found co-laborers who have applied his method with success.

The method may take different forms, where the measurements of the two coördinates of each star combine in different ways: this is not the place to dwell upon the variations which the inventor studied in detail. We will simply call attention to the fact that the advantages are still greater as regards the absolute declination than for the right ascensions, since these measures are subject to a special error, refraction, and the systematic variations of this refraction between day and night, between summer and winter. The method of M. Loewy permits of eliminating it.

In these later days Loewy busied himself with the study of the circle divisions; this study always represents a long and difficult work which can be shortened without the exactness suffering, if the measures are intelligently directed in such a way that the determination of the different lines of the same order are given the same weight. Much time and trouble can be economized by following a judicious course in this work; how to choose this course is the problem which Loewy had proposed to himself and which he had

happily solved.

I cannot speak of the daily tasks at the Observatory, but simply recall the part which he took in the determination of the differences of longitude between Paris and Marseilles, Marseilles and Algiers, Algiers and Paris, Paris and Berlin, Paris and Bonn, Paris and Bregenz, Paris and Vienna.

But that which occupied him almost exclusively in his last years, and which will remain one of his best titles to renown, is the photographic atlas of the Moon which he executed in collaboration with M. Puiseux. He had created the instrument; it was the great bent coudé telescope; this apparatus, thanks to its great focal length, gave a direct image of unusual dimensions. This image was further enlarged afterwards. Before reaching the perfection which we admire in the plates of his atlas he had to overcome many difficulties.

However numerous may be the published plates, they can give no idea of the immensity of the work accomplished. Without doubt they observed only when the images seemed good so as not to spoil the plates; but nevertheless they were obliged to throw away nine out of every ten photographic plates, so as to keep only those whose sharpness was perfect. The two authors allowed themselves absolutely no retouching. It is only at this price that a document having scientific value can be obtained; hence, what a precious source of information are these perfectly true photographs, taken at all phases and consequently under all degrees of illumination. A few years from now we shall be able to know without doubt whether our satellite is congealed in positive immobility, or whether rare changes take place there, as has been sometimes affirmed without any proof other than the fancy of a draughtsman.

Messrs. Loewy and Puiseux tried to get from their plates all the information possible; they wished to know what they teach us about the history of the Moon. This heavenly body, today reduced to silence and the repose of death, has in fact had a history; it was once alive and it is impossible not to recognize the traces of the great cataclysms of which it was formerly the theater. Deprived now of atmosphere, it may have had one formerly; and Messrs. Loewy and Puiseux are disposed to think so, for they believe they can see the effects of the winds which blew about it at some anterior epoch.

In 1878, after the death of LeVerrier, Admiral Mouchez appointed director of the Observatory, had Loewy join him as

assistant director; he held the same position under the direction of Tisserand. In 1896 after the death of Tisserand, so premature and so lamented, it was to him that the minister intrusted the direction of our great astronomical establishment. He had to divide his time between personal research and the duties of administration. These constant cares took a part of his days while his nights remained devoted to scientific work. His directorship was fruitful and, to confine myself here to material progress and to that which has a direct scientific interest, I will remind you that he helped to have introduced into the observatory the printing chronograph and the self-registering micrometer, constructed by Gautier for meridian observations.

His authority among the French scholars went on increasing from day to day, but it was no less recognized abroad than at home. It became more firmly established on the occasion of two great international enterprises. The first had begun before him. I mean the Catalogue and Photographic Chart of the Heavens. It was Admiral Mouchez who had conceived the first idea of it, and Tisserand had continued the work of his predecessor. But fortunately his death caused no delay in the work, and thanks to Loewy, France retained in the international collaboration, the place which was due to its initiative. In the councils which meet periodically, the influence of Loewy was very great, and thanks to the authority which he was able to gain, it made itself felt very usefully in the intervals of the sessions.

The success, henceforth assured, has shown in a striking manner what can be expected of an international coöperation well directed. So when the discovery of Eros caused astronomers to forsee the possibility of a more exact determination of the solar parallax, appeal was again made to this coöperation; for the task seemed to exceed the forces of a single nation. Loewy conceived the idea and knew how to get it adopted; a congress brought about a prompt understanding and this fine conception became practical; a plan of work was decided upon and it was Loewy who was intrusted to write up the instructions about it which appeared in this publication; the greater number of the observatories of the world responded to this call and they have furnished us with a rich material from the observations which it remains to work up; in a few years we shall know whether they come up to the hopes that we had entertained of them.

The role that Loewy played in these two enterprises witnessed to his authority in foreign countries and at the same time developed it. The Royal Astronomical Society of London, the Academies of Vienna, St. Petersburg, Berlin, Rome, Washington admitted him among their fellows and their correspondents.

Loewy was good, he knew no malice. An intense worker himself, he liked great workers; they alone could count upon his support.

Sometimes he made mistakes in his valuation of men; but he could always recognize his error and he always did it without mental reservation and without fake pride. But a few minutes before his death at the Council of the Observatories he was energetically defending an astronomer whom he had once mistrusted, and a member of the Council called his attention to it and congratulated him on this proof of impartiality.

For two or three years past his health had been impaired, but his zeal for work, his scientific activity, had not slackened. And so his co-workers, even his family, did not believe his end was so near. On October 15, 1907, he went to the Council of Observatories which was to present for the choice of the minister a list of candidates for the directorship of the observatories of Algiers and of Marseilles. He began to speak, giving a remarkably clear exposition of the claims of the different candidates. He was speaking with some animation when all at once he grew weak and immediately lost consciousness. In a few moments he ceased to breathe; this fine intellect had been suddenly extinguished. French Science had received one more sorrowful blow.

Translated by Miss Isabella Watson, Northfield, Minnesota.
BULLETIN ASTRONOMIQUE, November 1907.

THE PLANET NEPTUNE.*

EDWIN HOLMES.

Numbers of people who, like myself, use a telescope as a hobby or a recreation, are troubled to recognize the planet Neptune, and have vague or erroneous ideas as to his appearance and dimensions. I have thought it might be an

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November 20, 1907.

advantage to collect in one place such data and information as I could find bearing upon the subject.

I fear vain expectations have been raised in many minds by the perusal of Mr. Proctor's statement in his "Half Hours with the Telescope" that he was certain no observer could mistake him for a fixed star with a 2-inch aperture and a few minutes' patient scrutiny in favorable weather. It is unnecessary to say anything more of this than our first President said he was afraid his late lamented friend must have drawn on his imagination with reference to what could be perceived upon this planet with a 2-inch telescope. I have seen a telescope on Parliament Hill which might show something extraordinary upon Neptune, since I heard its exhibitor state that it showed hundreds of stars with rings like Saturn, and that Capella had thirteen, all of different colors, which he would show after dark when Capella would be rising in the north. This was on the 28th of April, when I thought Capella was in another direction, but that is no matter.

Another statement that has misled many is that when Dr. Galle picked up Neptune he ejaculated "My God in heaven, this is a big fellow!" When I some years ago applied the word apocryphal to this anecdote, the late Mr. Sadler was very indignant, and said he had the story from one who was present. I am willing to qualify my expression and call it ridiculous instead, for what are the facts? Grant's history tells us that Dr. Galle compared the indicated part of the sky with a star map and found the latter did not contain an eighth-magnitude star, which was very near the position indicated by Le Verrier. The observation of the following evening showed the star had moved, and thus decided that this was the Trans-Uranian planet. There is nothing about its big disk. If the detection had been by means of its disk, it is plain its identification would not have had to wait for a second comparison with the map. Professor Challis also had recorded the position of Neptune as a star on the 30th of July and 12th of August, 1846 without his recognizing that it was anything other than a fixed star. It was not till September 29, after receiving other information, that he found one star among 300 examined presented some appearance of a disk. And as these observations were made with two of the largest telescopes then in

existence, one being $9\frac{6}{10}$-inch, and the other 11½-inch, it is certain Neptune presented no very striking appearance.

There is another story told by Mitchel in his "Orbs of Heaven," not of his discovery, but of his first recognition of the planet. After relating that Dr. Galle found an eighth-magnitude star which was not present on an accurate map of the Heavens, he says the following evening was awaited with the deepest interest to decide by actual motion of the suspected star whether it was indeed the planet. He relates that having obtained from Le Verrier the computed place of the planet, although he had no star chart to guide him and no meridian instrument, he hoped the power of his great telescope, 11-inch, might be sufficient to select it by the magnitude of its disk. He then says: "On placing my eye to the finder, four stars of the eighth magnitude occupied its field. One of them was brought into the field of the large telescope and critically examined by my assistant and rejected. A second star was in like manner examined and rejected. A third star rather smaller and whiter than either of the others was now brought to the center of the field of the great telescope, when my assistant exclaimed: 'There it is! there is the planet! with a disk as round, bright, and beautiful as that of Jupiter!' There, indeed, was the planet, throwing its light back to us from the enormous distance of more than 3000 millions of miles, and yet so clear and distinct, that in a few minutes its diameter was measured and its magnitude computed."

This is definite enough, but curiously in contrast with the other accounts of first night's performances, except Mr. Sadler's. I suspect both these stories have a common origin. Certainly, with a disk as round and bright as that of Jupiter, there could have been no hesitation at all as to differentiating the planet from a star, and with so many indefatigable searchers of the Heavens it must have been discovered long before.

But in addition, the first human eye to see the planet appears to have been that of Lalande on the 8th and 10th of May 1795. The planet had moved in the two days, and Lalande supposed his first observation to have been an error and thus missed. Professor Newcomb says: "A discovery which would have made his name immortal." I presume he has made his name so far immortal on other grounds that

it will never be missing from astronomical literature, but the point I wish to emphasize is that Lalande evidently saw nothing in its aspect to distinguish the planet from a fixed star.

Before citing the observations of various eminent astronomers I will just refer to the very various determinations of the diameter of Neptune. I shall append a list of all I have found to my paper but do not propose to read it. The diameters vary in arc from 2″ to 3″.12, or in miles from 27,000 to 38,265. When we note the great variations and reflect that one-tenth of a second at the distance of Neptune is equal to 1,370 miles, we feel some of the computations are a little wire-drawn.

I begin with observations made with the smaller instruments. "A Fellow of the Royal Astronomical Society" wrote that he had never been able to make anything of Neptune as a telescopic object, but that a power of 400 on an 8½-inch mirror *ought* to raise a small bluish disk, and that a 4-inch with 300 suffices to show it is not a star, and that is all. A 12-inch mirror ought to show a good disk, but to measure its diameter would require a large amount of skill and practice. I infer from his language he had not personally seen Neptune with either of the larger apertures, so that his remarks thereon are of the nature of pious opinions.

Mr. Herbert Sadler, with 430 on 6½-inch, saw a disk clearly. Kaiser, with 6½-inch refractor, measured the diameter to one hundredth of a second. Hind, with 320 on 7-inch, measured to the same accuracy. Dr. Terby, in 1889, wrote (in French): "With my 8-inch Grubb refractor, Neptune shows as a very little sharp disk, with powers of 150 and 250, but I have never succeeded yet in seeing it neatly with higher powers. These, however, show it very differently to a star." Mr. George Hunt says: "I have frequently examined Neptune with my equatorial of 8-inch aperture, but I have never satisfied myself that I saw a disk, though the planet shone with a steady light different from the constant flicker of a star." Mr. Gaudibert says: "I looked at the planet last night, January 6th. I saw at once it was not a star. Its appearance was like the preceding star, slightly out of focus, but not quite so bright. On the other hand, it was more steady, but the border was not sharp." This was with a 10¼ "With," and power 600 and 750. In another place, he says a star is a minute dot, and with the same focus

the planet has so sensible a disk that it is at once seen it is not a star of any magnitude whatever. Vogel, with the 11½-inch Bothkamp and 620, says: "Edge was not distinct; Neptune was exactly like a faint planetary nebula."

Dr. C. W. Wirtz, of Strassburg Observatory, obtained a determination of the diameter of Neptune with the 18-inch refractor of the Imperial University. He thinks it necessary to add that, with his instrument, the disk of the planet was not sharply defined, but was apparently surrounded by a faint nebulous aureola, the breadth of which was about one-seventh the diameter of Neptune.

A writer in the "English Mechanic" declared his 12¼-inch was not large enough to get a disk sharp enough to measure. No more trace of a disk than an 8½-magnitude star in the field.

Lassell, with a 24-inch mirror and power of 1,027, remarks that the feeble light of the disk, and its minuteness, renders its accurate measurement a work of great difficulty.

I have been unable to find any description of the appearance of Neptune in the Greenwich 28-inch, but I infer it appears as a very sharp disk, because with 670 the difference betwixt the equatorial and polar diameters has been measured as 0″.004, and it must be very well seen to permit of such accuracy. With the Lick 36-inch the disk seemed to be perfectly round in all the observations, and no markings seen with a power of 1,000. The Concise Knowledge Library says that with this instrument and power it looks like a planetary nebula.

We have a fairly general agreement that the planet does not present a distinct disk, but is nebulous; that its diameter is betwixt 2″ and 3″, with a preponderance for the smaller figure, and that it is useless to expect to see a disk with small instruments or with less power than 500.

Against the statements that a clear disk was visible with 430 on 6½-inch and the exact measures with 6⅓-inch we have to contrast the fact that in 11½-inch it looks like a planetary nebula, in 18½-inch has no sharp outline, that it was minute in 2 ft., and a planetary nebula with 1,000 on 36-inch. When such a power as 430 was used on 6½-inch, it is obvious that a spurious disk would be formed by every star bright enough. This may account for the visibility of a disk to Neptune with such an instrument.

I have known an observer to try for years with a 4-inch

to distinguish Neptune from a star without success, except by its motion. In my own experience I have often observed very carefully, and with 300 on 12-inch. I can in no way distinguish Neptune from a fixed star of equal magnitude. With 500 I have no certainty of a disk. A 700 shows a looseness about it, and 900 brings out a nebulous appearance without definite edge, not unlike Σ 5, but less brilliant and far less well defined. I am quite prepared to accept Mr. Arthur Mee's statement "that no physical detail whatever has been observed on Neptune, and so far no marvel-monger has ventured to canalize his surface" although Professor See believes he has remarked faint equatorial bands on the disk of Neptune. The expression used is not quite satisfactory, for Professor See does not profess to have seen but "only believes he has remarked" the bands. He says also that "the diameter of the planet has shrunk with the increased perfection of astronomical measurement, and that it scarcely, if at all, exceeds 2″." I do not think it can exceed this.

For if a disk of 2″ diameter is viewed with a power of 900 it should look nearly as large as the full Moon seen with the naked eye, and if 2″.433 as the Lick measures make it, it should show considerably larger and ought to be seen easily instead of appearing minute.

But the question of light affects our appreciation. Neptune is 30 times more distant from the Sun than our Moon is. and, therefore, 900 times less brilliantly illuminated. Flammarion in his "Marvels of the Heavens," says, "the light and heat which it receives are 1,300 times less than that with which the Earth is enriched, so that no great difference can be noticed betwixt the day and the night of this distant planet." "The stars of the heavens remain visible day and night and the Sun is only a more brilliant star than the others." I do not understand his calculation, but the light being reduced to $\frac{1}{900}$ is still equal to that of 686 full moons, a rather respectable difference betwixt the day and the night. If we examine the planet with a power of 500, we spread the same light over 250,000 times the surface, and with a 12-inch telescope, which has sixty times the aperture of the eye, the light is increased 3,600 times *if* all the light reaches the eye, but no telescope transmits more than 80 per cent, so that 3,000 times is an outside estimate. Then multiplying 900 by 250,000 and dividing by 3,000 we find that viewed under these conditions the disk

of Neptune is 75,000 times less brilliant than the Moon to the unaided eye, and is only a feebly luminous object. With 700 the illumination is reduced to $\frac{1}{150000}$. Now, I have a little contrivance which loses light by four reflections at surfaces of plane glass, which I calculate reduces brilliancy to about the same proportion, and I find the Moon thus seen is a very indefinite object and looks small in consequence. I conclude the want of light accounts for the smallness of Neptune as seen in a telescope, but it also disposes of the "big fellow" idea.

Referring again to Professor See's measures, the BULLETIN OF THE ASTRONOMICAL SOCIETY of France for 1901 says, "this diameter and this density are less than has been previously admitted." There seems some error in implying that density and diameter decrease together, but the various densities are given evidently of little value, since with the same mass, with a diameter of 2″ the density must be 2½ times greater than with a density of 3″.12.

While on this point of possible printers' errors, I may mention that, in an old edition of Chambers' "Astronomy," there is an illustration of the perturbations of Uranus by Neptune, which shows both planets traveling in their orbits the reverse way to the other planets and to that we see in the northern hemisphere. This might hardly be worth mentioning but for a passage in the same author's "Solar System," which states that "great differences exist in the inclinations of the orbits of the different planets to the plane of the ecliptic, a fact which is better shown by a diagram than a table of mere figures. The orbit of Uranus is, indeed, so much inclined that its motion is really retrograde compared with the general run of the planets, and the same remark applies, though much more forcibly, to the case of Neptune;" and the diagram shows the Earth's orbit inclined 23½° to the ecliptic, and that of Neptune 146°.

It does not appear to me sufficient notice is given to the fact that Alexis and Eugène Bouvard had the idea, not only of an outer planet, but of its position and distance, before Adams and Le Verrier. They had even pointed out that the planets must have been in conjunction about 1822, and that the disturbing planet "must be at a distance from the Sun of less than 32 times that of the Earth from the Sun, and not less than 28 radii of the Earth's orbit;" and therefore the longitude of the planet January 1, 1847, would be be-

twixt 323° and 328°. The Bouvards were therefore very much nearer the real distance than either Adams or Le Verrier.

I have come across a recipe for the identification of planets in an old "Leisure Hour" which may interest: "First, planets are generally lower in the horizon than stars, and they generally rise and south in localities where stars are not numerous; second, that, if due in the evening, planets rise before stars, and that, in the morning, they shine after stars become invisible; third, that planets are larger and more luminous than stars." I do not know who was the writer, but he goes on to say: "Everything has its season, and star-, or rather, planet-gazing must also be prosecuted at proper times. Improper times are during the day and at midnight. During the day planets cannot be seen, owing to the superior light of the Sun. At midnight they can be seen, but it is wrong to encroach on the hours dedicated to repose."

Authority	Aperture of Telescope in inches	Power.	Diameter in Seconds	Diameter in Miles	Remarks.
F. R. A. S.	4	—	—	38,138	No disk.
Sadler.	6⅓	430	—	—	Net disk.
Kaiser	6½	200	2.87	—	
Hind	7	320	2.47	31,000	
Terby	8	150–250	—	—	Little, very sharp disk.
Hunt	8	—	—	—	Never sure of disk.
Gaudibert	10¼	600–750	3	—	Like star out of focus.
Encke	9 6/10	264	2.82	—	Bright line micrometer.
Galle	9 6/10	264	2.62	—	Bright line micrometer.
Encke	9 6/10	264	3.12	—	Bright field.
Galle	9 6/10	264	2.12	—	Bright field.
Madler	9 6/10	532	2.40	—	
Mitchell	11¼	—	—	—	Diameter stated to be equal to eight times that of the Earth.
Vogel	11½	620	2.66	—	Exactly like a faint planetary nebula.
Wirtz	18	—	2.303	31,506	Disk not sharp.
See	26	—	2.008	27,000	
Lassell	24	1,027	2.71	—	
Lassell and Marth	48	872	2.24	—	
Grenwich	28	670	2.096 2.1	—	
Lick	36	1,000	2.433	32,900	
Lockyer	—	—	—	36,620	
Chambers	—	—	2.7	37,500	An old Edition.
Neison	—	—	—	27,000	
Beckett	—	—	—	38,265	
Newcomb	—	—	2.56	34,500	

PLANET LAUNCHING.

THOMAS CURRAN RYAN.

FOR POPULAR ASTRONOMY.

The launching of a planet (or, the same thing, a planet's nucleus) in such a system as ours, which, Sun and all, is being translated through space, upon a highway as wide as the diameter of Neptune's orbit, and with a velocity of translation *along* that highway of about twelve and one-half miles per second, is a problem that was not involved in the cosmogony of Laplace. He assumed that the entire original mass possessed axial revolution, for which he did not account. If we accept his assumption, and if the whole mass had then the same translatory motion through space that it now has, it is easy enough to see that each particle in its equatorial zone, from center to circumference, extending beyond the orbit of Neptune, was occupying and pursuing just the *sort* of orbit in which the planets are now moving. So far as Laplace dealt with the orbits, his task was the simple one of gathering up bundles of these primeval orbits and binding them into sheaves, in his "rings." Thus everything was already launched into the present planetary orbits at the point where Laplace began his study which related only to the subsequent evolution. The assumed motions were not disturbed by the change from a revolving mass to revolving "rings."

But the new cosmogony, suggested by Messrs. Chamberlin and Moulton, begins farther back, and takes upon itself the task of explaining the phenomenon of planet launching, *a profound and curious problem, never before broached in astronomy*. When a longer and sustained inspection of it shall disclose to our minds some conception of its real immensity, perhaps we, like Laplace, may have to invoke chance. Dame Tyche was rather kind to Laplace. She gave him the revolving nebular mass that he needed, once out of half a billion of chances. At least we have his word for it. Agnes M. Clerke says, "human credulity is nowhere more conspicuous than in what it is prepared to attribute to chance." A wise and witty saying this—a thought well worth pondering. Chance is merely chaos. To invoke it is to acknowledge ourselves baffled. Chance and science are

diametrically opposed thoughts. If the new cosmogony is to explain how the planets were launched, it must do it upon scientific grounds. Otherwise it will be no improvement upon the "nebular hypothesis."

The fundamental idea of this new cosmogony (Chamberlin's "Geology" pages 1-81; Moulton's "Introduction to Astronomy" pages 440-448) is that the matter now constituting a planet was at one time a portion of the Sun's mass, and began its evolution into an independent globe through being ejected into space by the force of explosive eruptions, such as are now seen taking place in the Sun, only upon a vastly larger scale. Tentative preference is given to the supposition that the disturbances which caused these eruptions took place while another Sun was passing near enough to cause disruptive tidal protuberances in ours. The result is supposed to be a spiral nebula the nucleus of which later recondensed into our present Sun, leaving the "arms", "knots", and diffused materials of the nebula to be gathered up in the meantime into planets, their satellites, comets and asteroids.

The authors of this hypothesis make no dogmatic claims for it. They are testing it mathematically and mechanically, to determine its adequacy as a solution of the phenomena taking place in our solar system. A few tests of this sort have been made, with results which Mr. Chamberlin says are, upon the whole, encouraging. But, as I study the conditions opened up by the theory, they impress me more and more as involving an interminable range of inquiry.

It is, of course, assumed that the force used in the case did not "*scatter*." It fired its charges in certain *directions*. The ejected sun-lava was not sent forth in *spray*, but mostly in *streams*. Hence the "arms" and "knots" of the spiral nebulae. Quite naturally the "knots" are supposed to represent the *nuclei* of planets in this scheme. Nor is it difficult to account for them. Indeed, if we knew that the "arms" of the nebulae were caused by powerful explosions proceeding from the body of a Sun, we should be disappointed if the "knots" were not there. We should not know how to account for their absence. For, as the most forcibly ejected portion of this matter receded from the Sun it would exhibit a steady decrease in velocity, and this would result in an overtaking process, crowding the more rapid rear upon the slowing front which would thus become a sort of dam, impeding the stream. The less forcibly ejected matter thrown

out by the dying explosion would string out behind, forming the "arms."

No system of cosmogony is complete unless it will account for *all* the phenomena of our solar system—for all that the bodies belonging to it are now doing. And, so, the first thing to be considered is their present movements. We see a very nice balancing of antagonistic forces in our solar system. As an instance of this, all the planets are moving with greater velocity than the Sun, yet they do not out-run him. The Sun moves through space at the rate of about twelve and one-half miles per second. It is said in text-books that Mars moves about 15 miles per second, and the outer planets are given velocities that gradually decrease with distance, until we get to Neptune which we are told moves only about three and a half miles per second. But these velocities were ascribed before the Sun's motion was discovered. They are the velocities that would carry the planets around the Sun in their periods, if the Sun were a stationary body. Planets moving at less than the Sun's velocity could not keep up with him as ours do. The slowest of them must have a speed considerably greater than the Sun, inasmuch as they have to keep pace with him though they all travel very winding paths while his path is comparatively straight. The planets are moving in the same direction as the Sun, but they progress forward upon this road by swinging around the Sun's path, so that their orbits are shaped like cylindrical spirals,—i. e. like corkscrews. *They move ever forward upon a spiral curve.*

In examining the problem of "launching" a planet, under the "planetesimal hypothesis," (as Mr. Chamberlin calls it) it will simplify the discussion to confine ourselves to one planet. They are all governed by the same laws: all their movements teach the same lesson. What is our planet doing? It turns upon its axis, but it is not assumed *here* that this motion affects the question of launching, inasmuch as several bodies of the system are able to hold their orbits though they have no true axial motion. We only need to inquire: Could our planet have been driven out *from the Sun* to a distance of 93,000,000 miles, into its present orbit? If that could have been done, state *how*. Show that the two motions—the 12½ miles per second motion, toward the north inherited from the Sun, and the added motion it received as a projectile shot away from the Sun (of which it would seem

it retained the large excess over and above the Sun's velocity, which it now has) could in some way be combined to give that nucleus the Earth's present impulse of simple motion—i. e. to give us that straight line of motion we should now have if the Sun were to stop attracting us. This is what I mean by "launching." The planet pursues its course: it has its orbit. It was launched into that orbit some time. How?

And, right here, I beg the pardon of better men than myself, for making a suggestion. Shall we ever begin to think quite clearly about celestial mechanics so long as we persist in saying that a planet has *two* motions, one forward with the Sun, the other a circle around the Sun? No thing can have two simultaneous motions, for it cannot occupy two parts of space at the same moment. The two-motion idea seems to represent the fading inertia of the old "ring-orbit" around a *fixed* Sun. Our orbit is undoubtedly a compound of initial motions; but, whatever diverse impulses were involved in its initiation, the result could not have transcended the laws of motion, consequently, the initiatory impulse must have been a straight line. *It must have been a tangent of the orbit.* It was that straight line which the Sun's attraction is *now* constantly bending into the series of infinitesimal straight lines which in the aggregate make the curve our planet moves in. Could the Sun's attraction combine with our velocity to *put us into* that orbit, and *keep us there*, if our planet, or the nucleus of it, had been expelled from the body of the Sun? In other words, does that straight line which now represents the tangent of the Earth's orbit proceed from the body of the Sun? Obviously it does not.

It may be found that the "planetesimal hypothesis" makes it necessary to assume that prior to planetary initiation the Sun must have been much more massive than now, for the process seems to me to involve a vast amount of waste. But this, if true, would not appear to alter the launching problem. It could only affect the space *dimensions*, not the space *form*, of the problem, nor the laws invoked into effect by the conditions, any more than a geometrical figure can be changed by simply increasing or decreasing the length of its lines without altering their geometrical relations or proportions.

In examining the mechanical features of the launching problem, let us, for simplicity, call that face of the Sun toward

the solar apex, the "front", the opposite face the "rear", and the plane midway between these the "flank". Suppose, then, that the nucleus of our planet was ejected straight from the front. If this mass was thrown off with anything less than a "velocity of escape", must not the Sun eventually, aided by attraction, overtake and re-absorb it? On the other hand, suppose this mass to have been ejected from the rear. Should we not have the same two alternatives? Again, if the supposed "nucleus" were expelled from the Sun's flank, upon a line perpendicular to his path and intersecting his center, the ejected mass would inherit the Sun's motion assuredly, so that, however forcibly it was expelled, it would always occupy a line perpendicular to the Sun's path and intersecting his center. If it had a velocity of escape, it would escape on this line. If it had not such velocity it would gradually approach the Sun upon this line and be reabsorbed by him. Must it not be so with all masses of matter ejected from the body of the Sun, no matter in what direction? Ever and always, however their direction of flight may be affected, by attraction of, or collision with, other bodies similarly ejected and flying from the Sun, they are affected by only these two forces, the force driving them directly away from the Sun, and the Sun's attraction pulling them directly toward him. The stronger of these forces they must obey. *The angle upon which they move outward is the only road they can possibly travel.* With a velocity of escape they will escape from the Sun upon that road; with a less velocity they must return to the Sun upon the same road. A planetary orbit does not seem to me to be a possible result in such a case.

What would be the Earth's motion now if the Sun were to stop attracting us? The Sun is not going to stop attracting us, but the question, what would our motion be if that attraction should cease, is relevant, because his attraction is responsible for the fact that we move through space in a spiral instead of a straight line. The straight line in which the Earth would now move if it were released from the Sun's attraction, is a tangent of the present orbit, and so, it is not a line proceeding from the body of the Sun. How then can we conceive it as evolved from a force proceeding outward from the Sun? How are we to account for a straight line of motion outward from the Sun being changed into our present simple impulse of motion from no

cause but the Sun's attraction? Could attraction do that? Obviously, attraction is not exhibiting any force *now* that would be competent for such a task. Did it once do a thing which it could not do now? If not, then we must eliminate as impossible for evolution into planetary orbits, all motions derived from explosions sending the ejected matter in any direction whatever outward *from the Sun.* In other words, our planet, or its nucleus, could not have been launched by this means *alone.* It may have been thus driven to a point from which it could be launched. But the real launching must have taken place somewhere near the planet's present mean distance from the Sun. As the Sun's attraction, at this distance, is only strong enough to *hold* our planet in its present orbit, it seems that the process of launching the planet involved such a change in the angle of direction of simple motion as would task the Sun's attractive energy with just this *holding* of the planet, this being *the limit of its power.* It is an interesting inquiry—what angle of direction from the Sun's motion would do this. But if we knew precisely the distances, velocities and respective masses of Sun and Earth we could locate the line. Let us content ourselves, for the time being, with the fact that this angle, whatever it is, was found, since the Earth is doing what it could not do otherwise. What force can the new cosmogony invoke to join with those already mentioned in such manner as to impart this essential change of direction to our planet's motion, after its nucleus was expelled from the Sun's mass? Could it have been caused by the attraction of a "stranger" Sun, supposed to have been instrumental in increasing the violence of the explosions? The change needed is not to move the first line of motion of its nucleus to a line proceeding in some other direction, but still from the Sun. Impact with or attraction from other outgoing masses could only do that. What we need, is a line of motion for the "nucleus" that shall cross in a diagonal direction a line running parallel with the Sun's path and about 93,000,000 miles distant from that path. If the direction of the Earth's simple impulse of motion, i. e., the motion it would have if released from the Sun's attraction, were turned to a larger angle from the Sun's path than the present tangent of our orbit, we could not have a planetary orbit, for our speed is too great. If it were turned to a lesser angle with the Sun's path, we could have no planetary orbit, for

our speed is not enough. Our speed is sufficient to give us a planetary orbit, only at our present distance from the Sun, not at a greater or less distance. This is evident from the fact that the velocities of all the planets decrease with increasing distance and increase as they are located nearer to the Sun.

Is gravity the sort of force that could perform the task of launching into a planetary orbit a volume of matter ejected from the body of the Sun? If our Sun or masses of matter flying away from it could not do this, could the supposed "stranger" Sun, by its attraction, do it for us? That attraction would bend our first line of motion toward that Sun. This means that ejected streams of matter upon the side nearest that Sun would be bent farther away from our Sun's path. This would only increase the difficulty. But streams upon the opposite side would be bent toward our Sun's path. It would be possible, therefore, that the necessary change of direction might be thus given to some of these latter, were it not for the fact that attraction never stops pulling. *The proper direction once imparted to the planet's nucleus must be thereafter left undisturbed.* But if the attraction of another body pulled the Earth or its nucleus into the right path, it would keep on pulling, and *immediately* take it out of that path again. Here we meet with a task which it seems such a power as attraction is not fitted for. The sort of help needed to launch into a planetary orbit, matter thus ejected from the Sun, would seem to be a sudden impact that does all it can do *in a moment* and has no further efficiency. Such help might come from the "carom" of an outgoing mass upon an infalling mass. To suppose a case: if the nucleus of our Earth were ejected from the Sun, but when it reached the present distance, or a little more, it collided, at the proper angle, with an infalling mass from the "stranger" Sun, or with one from our own Sun which had met with no obstruction, and had consequently traveled farther before the Sun's gravity overcame its outward impulse, such an impact might be competent not only to change direction but to give to the masses thus united a new impulse of velocity in the new path. If the two suns passed near enough to each other so that the streams of matter ejected from both became intermingled with each other, and if there were "A succession of more or less irregular outbursts", as Mr. Chamberlin suggests, and

as it seems there would certainly be, the sort of "caroms" that would be required to impart the needed change of direction would occur much more frequently. For, as these suns must move either in different directions, or upon parallel lines, and, for the time being, presumably upon curved paths, on account of their mutual attractions, no two of these "successive outbursts" would be projected at the same angle. Here we should have, in great profusion, the multitudinous diversity of angles of motion needed for *attempts* at launching. And if these favorable conditions lasted long enough to call forth from the "womb of time", the number of successful efforts our solar system proves to have taken place some time, and in some way, we could thus account for what we see.

As to the length of time such favorable proximity of suns might last, all would depend upon their respective courses and velocities. Three assumed cases will illustrate this: suppose two suns each having a velocity of thirteen miles per second to be passing each other moving in opposite directions, upon parallel paths. Suppose their nearest approach to each other to be seven billions of miles and that they could cause eruptions of the required force in each other at a distance of nine billions of miles. They would remain within this disruptive influence about ten years. Suppose, again, that they were moving upon parallel paths and in the same direction, one at thirteen miles per second, the other at twelve and a half, they might be together long enough to disrupt each other entirely. Suppose again that their paths crossed at right angles; they might not remain long enough in each other's company to give birth to planets. If we should assume that our Sun was, in ante-planet times, much more massive than now,—that the forces which now expel eruptions of matter from him, sometimes with an initial velocity of 500 miles per second, and to a distance of 300,000 miles, were then much stronger, our Sun might do work of this kind without help, and time would not be an important factor in the problem. In either case the myriad probabilities of failure against success in launching the nucleus of a planet, and the certainty that the failures would result fairly often in expelling masses of matter with a velocity of escape, would, if we accept the new cosmogony, lead us to look for evidence, in our present solar system, of the former existence of a large amount of mass and energy which has been lost in abortive efforts at planet launching.

Wausau, Wisconsin.

AN ASTRONOMICAL THEORY OF THE MOLECULE AND AN ELECTRONIC THEORY OF MATTER.

Solar and Terrestrial Physics Viewed in the Light Thereof.

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FOR POPULAR ASTRONOMY.

Part II.

SPECIFIC STATEMENTS OF FUNDAMENTAL CONCEPTS CONCERNING THE CONSTITUTION OF MATTER.

It is a matter of common observation, as well as a scientific fact, that all known matter exists in one or other of, at least, three conditions, or states; viz., the solid, the liquid and the gaseous, and that these three are transmutable *inter se*. Solid matter can, by the application thereto of heat to a certain degree of temperature proper to each substance and termed the "point of fusion", or the "melting point," be converted into the liquid state, as ice into water at 32 degrees Fahrenheit, steel at from 2400° to 2600° and platinum at about 3650°, while even that most refractory element, *carbon*, becomes plastic, and shows signs of melting in the heat of the voltaic-arc the temperature whereof is between 6000° and 7000° of the Fahrenheit scale. By the further application of heat to the liquids so formed from solid substances, up to the temperature of ebullition, or the "boiling-point", as it is commonly called—which for water is 212° Fahrenheit under the normal conditions—all these liquids can be transformed into vapors, or gases, the temperatures of ebullition ranging from far below zero, as in the case of liquid air, up to the temperature of the electric arc between 6000° and 7000° as aforesaid, while carbon itself may be vaporized at about 7500° Fahr. Conversely, all gaseous matter, even the gases once regarded as inconvertible and hence called permanent gases—oxygen, nitrogen and the mechanical mixture thereof known as "atmospheric air," and hydrogen, have all been within comparatively recent times, reduced by increase of pressure and decrease of temperature, not only to the condition of liquids but also to the solid state, by a further abstraction of heat from the liquids under pressure, by the sudden expansion of a portion of the compressed fluid into the atmosphere, whereby *work* is performed by the expanding mass, and a definite equivalent of the correlated energy-heat abstracted therefrom.

Scattered over the expanse of the heavens above us are to be seen faintly luminous, hazy objects—mere wisps, or patches, of nebulous light—many of which, as the telescope has demonstrated, are simply clusters or groupings of distant stars *apparently* so close together that the light of each individual star of a cluster, overlaps that of its immediate neighbors in the group, the really widely separated stellar points of light being thereby blended into the nebulous object aforesaid, this effect being caused by "irradiation," an optical phenomenon whereby a very small surface heated to incandescence *appears* much larger than it is *in reality*, as is well illustrated by the incandescent electric lamp, the extremely thin filament whereof appears very much thicker while the electric current is coursing through it, than it does when the light is extinguished, and if two such filaments—or portions of the same filament—were set parallel, at a very small distance apart, (less than one-twentieth-inch) the light of one would so overlap, or blend with, that of the other, that the two would appear as a single luminous thread.

This phenomenon has no objective existence, it being a purely physiological effect or sensation, caused by the action of very bright light upon the organs of vision, and when a sufficiently high magnifying power is used, these nebulous objects are resolved into separate stars, a notable group of this kind, distinctly visible to the naked eye—and sometimes mistaken for a comet—being that known as *Præsepe* in the constellation Cancer, but many other of these nebulosities cannot be thus resolved into discrete stellar points, even by the highest power of the greatest telescopes, nor can they ever be resolved by telescopic or other means because when viewed through the spectroscope, they exhibit spectra consisting of bright lines, which are the distinctive spectra of incandescent gases, and these objects are therefore, to be regarded as enormous volumes of gaseous matter—the true *nebulæ*—posited in space at vast distances from the Earth and from the whole solar system. These nebulæ are of various shapes and dimensions but some of them present a more or less distinct central condensation, or stellar nucleus, a fact which logically leads to the conclusion that these volumes of gaseous matter for ages have been—and now are—undergoing, under the action of the force of gravity inherent in the mass, a gradual process of compression resulting in

the generation of heat in the condensing matter, a portion at least of which heat is continually radiated into surrounding space, thereby permitting the process of condensation to continue. This generation of heat by the compression of a gas is a matter of common observation as in the case of ordinary air compressors, the quantity of heat developed being proportional to the mechanical work done in the compression, and rigorously determinable by means of an algebraic expression of the thermodynamics of gases.

There is considerable—even quite sufficient—evidence in favor of the hypothesis that our own particular star—the Sun—is the result of such a process of compression or condensation (productive of the intrinsic heat of that body) whereby it has been brought to its present dimensions, density and temperature, from a vastly extended volume of gaseous matter primarily in equilibrium, as to density, temperature and pressure, with the superlatively tenuous, and extremely cold, matter called the "luminiferous ether" which fills all interstellar space and of which, indeed—for reasons to be stated subsequently,—this solar nebula may be regarded as a derivative. In order that this process of condensation should continue—or even begin—it is necessary that a portion at least of the heat developed by the compression due to gravity, be simultaneously radiated from the surface of the gaseous nebula into surrounding space, otherwise the expansive effect of the heat so developed would counteract the compressive force in such a mass of tenuous gaseous matter. If *all* the heat were radiated from the surface of the mass, as soon as generated, the condensation would proceed *pari-passu*, but if—as seems to be the case—compression and the consequent generation of heat, operate more rapidly than does radiation, the condensation of the nebulous mass will be checked at certain definite and—as will be shown subsequently—definable intervals and a force antagonistic to gravitation be developed so that when radiation has had time to operate sufficiently, and the process of compression is resumed, a portion of the superficial matter of the nebula, particularly that in the region of greatest angular velocity (all such condensing masses must ordinarily be endowed with some motion of rotation around an axis) will be separated from the main mass, and be left behind, the aforesaid force opposed to gravitation acting conjointly with the centrifugal force due to rotation, in effecting the fission, and the diffused gaseous matter, thus separated, will be suspended in space above, and en-

circling, the surface of the parent mass around which it will revolve in the direction of the rotation of the nebula whence it has been derived.

As this matter is obviously not of equal density throughout, minor centers of condensation toward which surrounding gases will gravitate must be the result, each forming a nucleus toward which compressive action will be exerted by the force of gravity resident therein—just as in the case of the main mass of the nebula that is now the Sun—these secondary centers, or nuclei, being the embryos which have developed into the planetary bodies which must revolve around the Sun in the direction disclosed by astronomical observations.

The detached masses consisting as they do of very tenuous diffused matter, may be roughly annular, but it is quite unlikely that, if viewed from a very distant standpoint, they would appear as well defined luminous rings like those of the planet Saturn, and it would seem that a quite perfect analogue is to be found in the great nebula in the constellation Andromeda, clearly visible to the naked eye and known from the earliest historical times, with its central condensation, or nucleus, and extended oval surrounding nebulosity divided by dark rifts into several nebulous belts wherein appear a number of minor centers of condensation, or nuclei, as shown in Robert's fine photograph of this nebulous object. While the character of this nebula as viewed spectroscopically, is somewhat dubious—its spectrum being, as remarked by an eminent astronomer, "perfectly expressionless—and while it *appears* to have been partially resolved into stars, it certainly presents the aspect of a vast gaseous nebula condensing toward the stellar or the sunlike condition, in the manner above described. The stars into which a portion of this nebula *seems* to have been resolved may in *reality* be extraneous to, and far beyond the nebula itself through the tenuous matter whereof they are visible—just as far distant stars have been seen to shine through the diaphanous "tail" of a comet—while the degree of condensation of the nebulous mass and the intrinsic physical conditions, may operate to prevent the exhibition, thereby, of a distinctive nebular spectrum or of a decidedly stellar one; but, in any case, this nebula serves well as an illustration of the appearance of the solar system in the early stages of its development, and in this connection a pertinent fact may be adduced. In the course of an investigation concerning the development of the solar system from the primitive nebula, by

means of analytical methods and formulæ based upon the principles of my general theory of gases, I found that, when this nebula began to differentiate from the ether, its radius—regarding the mass as spherical—was 11,600 times the mean distance of the Sun from the Earth, or nearly 400 times the mean distance of the planet Neptune, while if we regard it as an ellipsoid similar to the Andromeda nebula, its semi-major axis would be about 40,000 times the mean distance of the Sun. Now it can be demonstrated that if the Andromeda nebula is at a distance from us, equal to that of the nearest fixed star, α Centauri—so far as known—the annual parallax whereof is nine-tenths of a second of arc, and distance therefore very approximately 21 trillions of miles, its longer diameter must be more—probably much more—than 30,000 times the mean distance of the Sun from the Earth, which fact makes the analogy between that nebula and the primitive solar nebulous mass the more striking; this fact furthermore, indicates roughly the distance of that beautiful and wonderful celestial object that has been called Queen of the Nebulæ. To some readers it may appear that the foregoing paragraphs concerning the development of the solar system are tantamount to a re-statement of the Nebular Theory advanced by Laplace and other men of science, long ago, but such is not the case; there are several important points of difference, one of them being that said nebular theory postulates a primitive mass of *fiery* matter, whereas, according to the hypothesis that I have set forth, the matter of the gaseous nebula, when it began to differentiate from the ether and to condense toward its present state, was at the temperature of the ether or as it is sometimes called improperly the "temperature of space" of which several estimates have been made, differing among themselves, but to which physicists are generally agreed in assigning very low values—a determination based upon the principles of my theory, and which will be discussed subsequently, giving only a very small fraction of one degree above what is known as "absolute zero" which is nearly 460° *below* the zero of the Fahrenheit scale. The nebulous matter became heated only after a very considerable compression, the augmentation of temperature being very small until the volume of the nebula had contracted to nearly the present dimensions of the Sun the maximum internal temperature whereof, obtained by my method, being 13,400 degrees of the absolute scale, which temperature is only one-third of that to which

the matter would have been raised by *adiabatic* compression of the whole nebula from its outer limit at the distance of 11,600 times the mean distance of the Sun from the Earth (which mean distance is in round numbers 93 millions of miles) down to the present semi-diameter of the solar globe—432,200 miles—the other two-thirds having been lost by radiation into surrounding space, during the process of contraction, agreeably to a certain law that will be set forth in another part of this paper. Even when at the mean distance of the Earth, the matter that was to condense in like manner to form our globe, was at a temperature of only about four-tenths of a degree above absolute zero, and it did not rise to even one degree above, until the radius of the solar nebula had contracted to nearly the mean distance of the planet Venus.

For the absolute surface temperature of the Sun, at the present time, I have obtained a value of very nearly 6700 degrees Fahrenheit by means of four independent methods, the first employing as a *known* quantity the radiation of *heat* as indicated by the "solar constant" determined by the pyrheliometer; the second using the radiation of *light*, as measured by the candle-power of the luminous radiance emitted from unit surface of the Sun's disk, as the photometrically observed quantity; the third method is by means of the observed difference between the length of the solar spectrum and the length of the spectrum given by the voltaic arc as measured in the direction of the ultra-violet end, this involving the question of the physical properties of the luminiferous ether and its function in the propagation of radiance according to the principles of the "wave theory;" while in the fourth method, what is known as the "strength of pole" in the case of terrestrial magnetism is used as the known or observed quantity, terrestrial magnetism, for reasons to be stated subsequently in full, being regarded as due wholly to the radiant energy of the Sun, which has heretofore been regarded merely as a perturbing factor in the terrestrial magnetic field.

The analytical expressions for the law of radiation of both heat and light, whence I have obtained the aforesaid value of the surface temperature of the Sun—and also the density of the luminiferous ether relative to that of atmospheric air under the normal conditions—have been rigorously derived from the principles of my general theory, and involve the molecular and atomic weights or masses, dimensions and motions of the atoms in their orbits in the surface of each molecule of which

they are the constituent parts, in *absolute measure*, and those expressions have been crucially tested by comparison with radiation actually observed, and directly measured in *absolute units*, from divers material surfaces the temperature-excesses whereof above the temperature of the inclosure, or surroundings, ranged from comparatively low values—as from 10° to 200° for cast-iron and wrought-iron heating pipes—up to nearly 3300° Fahrenheit in the case of radiation from the carbon filament of an incandescent electric lamp, the quantity of heat or thermal energy, radiated from the iron, in thermal-units, per unit surface, per second, for the temperature differences, and also the number of thermal units and the corresponding number of electrical units (watts) radiated from the carbon filament, being in exact agreement with the values observed or measured and, in so far as I have been able to learn, no other "law of radiation" or algebraic expression therefor has heretofore been found competent.

The solar surface temperature that I have thus obtained is considerably lower than the experimentally determined value as found by Le Chatelier, which is about 13,700 degrees Fahrenheit, with a possibility of its being as low as 11,900 degrees, and also lower than that by Wilson and Gray—11,200° Fahrenheit—but the difference in each case is mainly, if not wholly, attributable to the fact that the deduction of temperatures as high as that of the Sun, by these experimental methods, is necessarily made through an extrapolation that requires the use of an adequate algebraic expression for the true "law of radiation," while this law itself, for the case of very high temperatures, has not been regarded as well determined, and purely empirical formulæ have necessarily been used, Le Chatelier employing an expression devised by himself while Wilson and Gray used Stefan's "law of the 4th power of the absolute temperature," which while analytically derived and answering in some cases of radiation quite well, depends upon certain fundamental assumptions not fully proven to be true. Le Chatelier employed the observed or measured intensity of the Sun's *light* in his investigation, and by using his observed values in connection with my analytically derived expression of the "law of radiation," I have found a value of the Sun's surface temperature *substantially the same* as that derived by me through the four independent methods as stated in a preceding paragraph; viz, 6700° Fahrenheit; in any case it is quite apparent that the enormous temperatures (some several millions of degrees) once

assigned to the Sun by physicists, and which were based upon laws of radiation such as Newton's "law of cooling" as it is called—which are true for only comparatively few degrees of temperature of a heated surface above its surroundings, can no longer be regarded as even approximately true. It should be noted that this solar radiating temperature—6700°—is the arithmetical mean between the maximum internal temperature —13,400° Fahrenheit,—according to my determination—and the temperature of the ether outside the solar globe, which temperature may in this case be regarded as "absolute zero."

The radiating temperature aforesaid is that of the surface of the photosphere or rather the upper portions thereof since we cannot well conceive that a volume of gaseous matter, such as the Sun, possesses a strictly *geometric* surface, this portion of the photosphere being immediately overlaid by the so-called "reversing layer" that comparatively thin stratum of glowing gases which, itself giving a spectrum of bright lines, strikes down, or absorbs, corresponding vibrations, or wave-lengths in the solar spectrum thus causing a portion, at least, of the dark lines exhibited by the latter.

In a paper published in the *Sitzungsberichte* of the Berlin Academy of Science, a translation of which was printed in No. 127 of Astronomy and Astro-Physics (August 1894), Professor J. Scheiner of Potsdam, Germany, in discussing the behavior of a certain line in the arc-spectrum of magnesium, in comparison with a corresponding line in the case of several classes of stars, states, as his conclusion, that in stars of Class II—with which the Sun is comparable—the temperature of the "reversing layer" is about that of the electric-arc, or between 5400° Eahrenheit and 7200° Fahrenheit, a mean value being 6760 degrees, of the absolute scale, which is almost exactly the value of my determination of the surface temperature of the Sun, by the several methods aforesaid, the difference being less than one per cent.

By means of an analytical method based upon the principles of my theory, and which will be fully set forth in a subsequent part of this paper, I have found that when the secondary "center of condensation," which formed the nucleus of the Earth, began to differentiate from the parent nebula, the temperature of its matter was only four-tenths of a degree above absolute zero, and its density only the 10-thousandth part of that of atmospheric air, under the normal conditions, and the radius of the volume of this fully expanded "terrestrial

nebula"—as it may be called—regarded as a sphere, was approximately 336 times the present semidiameter of our globe, or nearly 1⅓ millions of miles.

It appears that the development of heat in this secondary nebula by its compression or contraction under the action of its own force of gravity, in the same manner as in the case of the primary nebulous mass, was such that when contraction had proceeded to nearly the present dimensions of our globe, and the greater part of the condensing mass had become viscid or plastic, the temperature thereof was approximately 6300 degrees Fahrenheit, which temperature is still, according to my determination, that of the internal matter of the Earth at a depth of about sixty-five miles below the surface, the temperature increasing downward at the mean rate of about one degree for each fifty-five feet which is not only the value derived from many actual measurements, but also a theoretical value that I have obtained through an equation based on my theory, as will be shown hereafter. Notwithstanding the fact that the matter of the Earth below a depth of sixty-five miles is at a temperature of 6300 degrees Fahrenheit (nearly equal to that of the Sun's surface) throughout, it is demonstrable, for reasons to be stated, that this matter is *solid* and of very great rigidity, and we may regard it as a *nucleus* enclosed by a solid envelope sixty-five miles in depth, the temperature whereof gradually decreases upward to the surface, and if this envelope which is less than one-sixtieth of the radius of the Earth in thickness, were to be removed—like the rind from an orange,—the highly heated nucleus would glow with a radiance approximating that of the Sun's disk.

While it may seem reasonable to connect this high temperature of the interior mass of the Earth with seismic perturbations—earthquakes and volcanic eruptions—that during all historic ages have been known to manifest themselves upon the surface of the globe, and of which we have had very recently appallingly violent exhibitions throughout the world, it can be demonstrated that, while there is, to a certain degree, such a connection, it is not *direct* but quite *remote*. The matter of the envelope aforesaid having a thickness of sixty-five miles and which, for convenience, may be called the "crust" of the Earth—although there is no very great mass of liquid matter in the interior of our globe necessitating the idea of a superincumbent solid crust—is of very low thermal conductivity and the flux of heat from the internal highly heated mass, or nucleus,

to the comparatively cool surface, and its radiation therefrom into outer space are very *slow*, as well as very *uniform*, processes, as will be demonstrated when I have set forth my development of an analytical expression for the "law of radiation", and some other all-important factors in this connection. We must, according to my hypothesis, regard the cooling of the *uniformly heated* nucleus as being effected only in what we may call the "interface" between it and the superincumbent envelope, or "crust," whereof the temperature is *variable*—decreasing toward the surface—a thin layer being thus as it were, *very slowly* taken from the nucleus and added to the "crust" above it, the former therefore decreasing in volume and the latter increasing. Geologists generally regard earthquake shocks as caused by the rupture or slipping of underground strata at a point—or along an axis—where the regular order of deposition thereof has been altered, and a condition of stress brought about, by the secular cooling of the globe or its loss of heat during long ages of geological history, this causing what are called "faults" in the rocky strata. But, according to the view advanced above, the contraction of the volume of the Earth by reason of the secular cooling of its mass, is confined to the superficial envelope that may be called the "crust" as aforesaid, and it proceeds slowly and uniformly *pari-passu* with the processes of conduction of heat from the interior of the Earth and its radiation from the surface thereof, so that seismic phenomena might well be expected to recur but rarely, at indefinite intervals, whereas their occurrence is comparatively frequent and of a quite well marked periodicity —as will be shown,—a fact which is antagonistic to the hypothesis that these phenomena are due *immediately* to the stresses caused by contraction of the matter of our globe, consequent upon the secular cooling thereof and which fact, in conjunction with certain theoretical determinations that I have made has indicated to me a more probable *immediate* cause of these phenomena, a knowledge of which may lead to ability to *predict* concerning them; but a discussion of this matter must be deferred to a subsequent part.

Another point of difference between the nebular hypothesis of Laplace and the one that I have set forth above, is that the former regards the separation of rings of matter from the surface of the contracting nebulous mass, as due to "centrifugal force" alone, whereas, according to the latter view, the fission is effected not only by that force but mainly by one developed

by the heating of this mass of tenuous gases, and operating directly against the force of gravity, especially at the surface of the nebula. The probable existence of such a force was pointed out by me, in an article concerning an apparent connection between the relative densities of the planets, and the relative times of their rotation upon their axes, which article was published in No. 75 of the SIDEREAL MESSENGER (May, 1889 and further referred to in a paper of mine that appeared in No. 105 of ASTRONOMY AND ASTRO-PHYSICS (May 1892) under the title Radiant Energy as a Probable Cause of the Solar Corona, the Comæ and Tails of Comets and the Aurora Borealis, this repellent force being the same that urges the carbon particles from the filament of an incandescent electric lamp, and deposits them upon the interior surface of the inclosing bulb—a phenomenon that may be termed the "radiation of matter"—finely divided particles of matter being radiated outward from the Sun's surface in all directions, and to vast distances therefrom to the Earth's orbit and even beyond it—this tenuous, but probably solid, matter giving the faint, continuous, part of the spectrum of the "corona," and being also a factor in the causation of the "zodiacal light" and that peculiar phenomenon, the faint luminosity sometimes seen in the midnight sky, "shining in opposition, to the Sun, wherefore it has received its appropriate German appellation, *Gegenschein.*

In regard to the "tails" of comets, the *modus operandi* of the Sun's radiant energy in repelling the tenuous cometary matter in a direction away from that of the solar globe, is quite obvious but, as to the aurora, certain developments along the line of my theory with respect to "terrestrial magnetism" and the part that the Sun plays in its causation give another demonstrable and more satisfactory explanation carrying with it some important disclosures concerning electricity and magnetism and the distinctive spectra of comets and auroræ but a discussion thereof must be deferred to a subsequent part of this paper.

In addition to the matter thus radiated outward from the Sun there is most probably, scattered in space throughout the original volume of the solar nebula, some *residual* gaseous matter in the primitive, roughly annular, conformations, portions whereof instead of condensing into the planetary masses and revolving in approximately *circular* orbits around the Sun, have formed into very much smaller masses, and fallen quite directly toward the center of gravity of the solar system, which

lies within the present volume of the Sun—revolving around it in extremely *elliptical* orbits,—in most cases apparently parabolic, these masses of the residual matter of the primitive nebula constituting what we now call the "comets."

According to this view, all comets may be regarded as *original* members of the solar system, and constituted from what we may term the "debris" of the primitive solar nebula the boundaries whereof extended, at least, to a distance of 11,600 times the Sun's mean distance from us, or to a distance of a million millions of miles, possibly to three or four times that distance. In this connection it may be well to state that some time ago I found upon examination of the "elements" of all the periodic comets in the catalogues at hand, that the aphelion distance in each case, fell well within one or other, of the many rings or zones of separated nebulous matter between the Earth and the outermost limit of the solar nebula, according to my computations, a very great number, for obvious reasons having their aphelion distance in what we may call the *Jovian zone.*

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To be continued.

PROPOSAL OF A NEW METHOD TO COMPUTE A PLANET'S ANOMALY AND RADIUS VECTOR.

FRANCIS RUST C. E.

For Popular Astronomy.

By Kepler's first law a planet's orbit is an ellipse, in the one focus of which stands the Sun and the first step in computing a planet's ephemeris is to determine its location in its orbit by polar coördinates as functions of time.

For this purpose a fictitious planet, moving in a circle with constant velocity, a "mean" planet, is introduced, its anomaly, the *mean anomaly*

$$m = \zeta t$$

is directly proportional to time. From this mean anomaly an auxiliary angle u the *eccentric anomaly* is derived by the equation

$$m = u - e \sin u \qquad (1)$$

and from u follows the true anomaly θ by the equation

$$\text{tang}\,\frac{\theta}{2} = \text{tang}\,\frac{u}{2}\cdot\sqrt{\frac{1+e}{1-e}}. \qquad (2)$$

The radius vector is determined by the equation

$$\rho = a\,(1 - e\cos u). \qquad (3)$$

To solve the equation (1) for u has been the cause for Lagrange to invent his famous method of reversing series and by the most elegant methods of calculation he deduces further θ and ρ directly as functions of the mean anomaly, that is as functions of time:

$$\left.\begin{aligned}\frac{\zeta}{a} &= D_0 + D_1\cos m + D_2\cos 2\,m + D_3\cos 3\,m + \ldots\\ \theta &= m + E_1\sin m + E_2\sin 2\,m + E_3\sin 3\,m + \ldots\end{aligned}\right\} \qquad (I)$$

In these series the coefficients D and E are depending on the numeric eccentricity e only and have to be calculated for each planet.*

Unfortunately the convergence of series (I) is but very weak, the computing of θ and ρ by same consequently a tiresome task. So offering an easier method, I dare to ask the question: "*Why not rather this way?*"

By the second of Kepler's laws, the motion of a planet in its orbit is such, that the area swept over by the planet's radius vector is proportional to time. The total area of an ellipse is:

$$S = ab\,\pi$$

or for

$$b = a\cos\kappa$$

$$S = a^2\,\pi\cos\kappa$$

consequently if we consider the semimajor axis of the planet's orbit units of lengths

$$S = \pi\cos\kappa.$$

Supposed now the planet's period be t units of time, the area covered by the radius during one unit of time is:

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* Besel has deduced formulas for ζ and θ, introducing his function

$$I_n\,(x) = \frac{1}{\pi}\int_0^{\pi}\cos\,(n\,\phi - x\sin\phi)\,d\phi$$

for n a positive integer and x positive fraction or integer. Here is no room for further explanation. See Todhunter's, Laplace's, Lame's and Bessel's functions.

$$\Delta S = \frac{\pi \cos \kappa}{t} \qquad (4)$$

and if we construct a table, which gives the area, covered by the radius vector as functions of θ, we may from such a table find θ by interpolation with no more labor than it requires to find for a given logarithm the corresponding number from the tables.

The construction of such a table requires the squaring of an ellipse, referred to polar coördinates, one focus the pole and the major axis the polar axis. The vertex next to the pole corresponds to the planet's perihelion and the equation is:

$$\rho = \frac{a(1-e^2)}{1+e\cos\theta}.$$

From this we derive by the general expression of the area in polar coördinates:

$$S = \tfrac{1}{2} \int \rho^2 d\theta$$

counting the area from the perihelion and taking the semi-major axis for units of lengths:

$$S = \frac{(1-e^2)^2}{2} \cdot \int_0^\theta \frac{d\theta}{(1+e\cos\theta)^2} \qquad (5)^*$$

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* The evaluation of the integral

$$I = \int \frac{d\theta}{(1+e\cos\theta)^2}$$

necessitates a complicated operation, which we arrange here, in order not to interrupt the text with it.

Substituting

$$e\cos\theta = u,$$

results

$$-e\sin\theta\, d\theta = du$$

and

$$\sin\theta = \sqrt{1-\left(\frac{u}{e}\right)^2}$$

consequently

$$d\theta = -\frac{du}{\sqrt{e^2-u^2}}$$

and transforms our integral to

$$I = \int \frac{-du}{(1+u)^2\sqrt{e^2-u^2}}.$$

from which is derived, performing the integration

$$S = \chi . \cos \kappa - \tfrac{1}{4} \sin 2\kappa \sin 2\chi . \qquad (6)$$

$\sin k = e$ and for the auxiliary angle is

$$\text{tang } \chi = \sqrt{\frac{1-e}{1+e}} \text{ tang } \frac{\theta}{2} \left\{ \text{(g) in foot note} \right. \qquad (7)$$

This last equation for tang χ permits a simplification. Making

$$e = \sin \kappa = \cos \mu, \quad \text{or} \quad \mu = 90^\circ - \kappa$$

makes

$$\sqrt{\frac{1-e}{1+e}} = \text{tang } \frac{\mu}{2} = \text{tang } \left(45^\circ - \frac{\kappa}{2} \right)$$

consequently

$$\text{tang } \chi = \text{tang } \left(45^\circ - \frac{\kappa}{2} \right) . \text{ tang } \frac{\theta}{2} . \qquad (8)$$

Formulas (6) and (8) are excellently adapted for computing the area swept over by the radius vector as a function of the anomaly θ, the calculation to be carried tabularly as shown below:

For rationalization we make

$$\frac{e-u}{e+u} = \frac{1-\cos\theta}{1+\cos\theta} = \text{tg } \frac{\theta^2}{2} = z^2 \qquad (a)$$

which brings as its consequences:

$$(e-u)\ (e+u) = e^2 - u^2 = (e+u)^2 z^2$$

or
$$\sqrt{e^2-u^2} = (e+u)\, z \qquad (b)$$

and also

$$e - u = (e+u)\, z^2$$

or
$$u = e \frac{1-z^2}{1+z^2} ; \qquad (c)$$

lastly from (c) by differentiation

$$du = -\frac{4\, e\, z\, d\, z}{(1+z^2)^2} . \qquad (d)$$

From equation (c) results

$$1 + u = \frac{(1+e) + (1-e)\, z^2}{1+z^2} \qquad (e)$$

and from (b) and (c) we derive

$$\sqrt{e^2-u^2} = e z \left(1 + \frac{1-z^2}{1+z^2} \right) = \frac{2\, e z}{1+z^2} \qquad (f)$$

1	2	3	4	5	6
θ	$\log \operatorname{tang} \frac{\theta}{2}$ $\log \operatorname{tang} \chi$	χ arc χ 2χ	log arc χ log A	log sin $2\chi^2$ log B	$A - B = S$
	$\log \operatorname{tang} \frac{\theta}{2} +$ $\log \operatorname{tang}\left(45° - \frac{\kappa}{2}\right)$ $= \log \operatorname{tg} \chi$		log arc χ + log cos $\kappa = \log A$	log sin 2χ + $\log \frac{\sin 2\kappa}{4} = \log B$	

The calculation in column (2) (4) and (5) demands in each case the addition of one constant logarithm to the logarithm of a certain function of θ, which is done by a "*movable logarithm*", namely, the constant logarithm is written on a slip to be held in place over the other sum and only the result written below it.

Here is a sample of the calculation for the Earth's orbit:

$$\kappa = \text{arc sin } e = 0° \; 57' \; 34''.48$$

80°	9.9238135	39° 31′ 41″.468	9.8387842	9.9920294	0.6898002
	9.9165397	0.6898970	9.8387233	7.9148755	0.0082200
		79° 3′ 22″.93			0.6815802

By the method explained above the values of S were computed for an interval of θ five degrees and the attached fragment of a table composed by interpolation.

Introducing the expressions from (e) and (f) transforms our integral to

$$I = 2 \int \frac{(1 + z^2)\, dz}{[(1 + e) + (1 - e)\, z^2]^2}$$

or extracting the factor $(1 + e)^2$ in denominator

$$I = \frac{2}{(1 + e)^2} \cdot \int \frac{(1 + z^2)\, dz}{\left(1 + \frac{1 - e}{1 + e}\, z^2\right)^2} \cdot$$

For abbreviation introduced

$$c^2 = \frac{1 - e}{1 + e}$$

our last equation, combined with equation (5) in the text results in

$$S = (1 - e)^2 \int \frac{(1 + z^2)\, dz}{(1 + c^2 z^2)^2} \cdot \qquad \text{(A)}$$

In this equation (A), the new integral

$$K = \int \frac{(1 + z^2)}{(1 + c^2 z^2)^2} = \int \frac{dz}{(1 + c^2 z^2)^2} + \int \frac{z^2\, dz}{(1 + c^2 z^2)^2}$$

θ	S	θ	S
75° — ′	0 638286	80° — ′	0.681580
10′	0.639727	10′	0.683025
20′	0.641168	20′	0.684471
30′	0.642610	30′	0.685916
40′	0.644051	40′	0.687362
50′	0.645493	50′	0.688808
76° — ′	0.646935	81° — ′	0.690254
10′	0.648377	10′	0.691700
20′	0.649819	20′	0.693146
30′	0.651261	30′	0.694592
40′	0.652703	40′	0.696039
50′	0.654146	50′	0.697486
77° — ′	0 655589	82° — ′	0.698933
10′	0.657031	10′	0.700380
20′	0.658474	20′	0.701827
30′	0.659917	30′	0.703234
40′	0.661361	40′	0.704721
50′	0.662804	50′	0.706169
78° — ′	0.664247	83° — ′	0.707616
10′	0.665691	10′	0.709064
20′	0.667135	20′	0.710512
30′	0.668579	30′	0.711960
40′	0.670023	40′	0.713408
50′	0.671467	50′	0.714856
79° — ′	0.672911	84° — ′	0.716305
10′	0.674355	10′	0.717754
20′	0.675800	20′	0 719202
30′	0.677245	30′	0.720651
40′	0.678690	40′	0.722100
50′	0.680135	50′	0.743549
80° — ′	0.681580	85° — ′	0.724999

and the first of these integrals to the right

$$\int \frac{dz}{(1+c^2 z^2)^2} = \int \frac{(1+c^2 z^2 - c^2 z^2)\, dz}{(1+c^2 z^2)^2}$$

$$= \int \frac{dz}{1+c^2 z^2} - c^2 \int \frac{z^2\, dz}{(1+c^2 z^2)^2}$$

consequently we get:

$$K = \int \frac{dz}{1+c^2 z^2} + (1-c^2) \int \frac{z^2 dz}{(1+c^2 z^2)^2}. \qquad \text{(B)}$$

Upon the second integral in (B) we apply the formula of reduction

$$\int \frac{x^p\, dx}{X^q} = \frac{p-1}{2c\,(q-1)} \int \frac{x^{p-2}\, dx}{X^{q-i}} - \frac{b}{c} \int \frac{x^{p-1}\, dx}{X^q} - \frac{x^{p-1}}{2c\,(q-1) X^{q-1}} \qquad \text{(C)}$$

for $$X = a + 2\,bx + cx^2$$

the deducement of which will be shown below and which reads for our special case, that is with

$$X = 1 + c^2 x^2$$

consequently with 1 for a, nought for b, c^2 for c, $p = q = 2$:

An example may illustrate the use of such a table, which depending on e only, will remain useful for many years after being once properly computed.

Whereas the Earth and the Sun always stand opposite each other in the ecliptic, the Sun's longitude exceeds the heliocentric longitude of our Earth for 180°, consequently the sum of the longitude of Earth's perihelion plus the true anomaly θ, as taken from our tables, increased for 180° must result in the Sun's geocentric longitude. From different sources* I could compile: Epoch 1901, January 1, 0.^{h} Greenwich M. T. (the beginning of the XXth Century.)

Heliocentric longitude of ⊕'s perihelion 101° 14′; annual tropic motion + 61″.7 [composed of proper motion (11″.46)

* The astronomical data are anything but harmonious. The layman mathematician becomes puzzled as to "Why compute for fractions of seconds if you differ for minutes?"

$$\int \frac{z^2\,dz}{1+c^2z^2)^2} = \frac{1}{2c^2}\int \frac{dz}{1+c^2z^2} - \frac{z}{2c^2(1+c^2z^2)}$$

which transforms equation (B)

$$K = \frac{1+c^2}{2c^2}\int \frac{dz}{1+c^2z^2} + \frac{1-c^2}{2c^2}\cdot\frac{z}{1+c^2z^2}$$

or integration performed:

$$K = \frac{1+c^2}{2c^3}\ \text{arc tang}\ cz + \frac{1-c^2}{2c^3}\cdot\frac{cz}{1+c^2z^2} + C \qquad (D)$$

By equation (a)

$$z = \text{tg}\,\frac{\theta}{2}$$

consequently z vanishes if θ is zero and no constant is to be added in (D). Resubstituting this value for z and introducing an auxiliary angle χ:

$$\text{tang}\,\chi = \sqrt{\frac{1-e}{1+e}}\cdot\text{tang}\,\frac{\theta}{2} = cz \qquad (g)$$

brings equation (A) to the form

$$F = \frac{1+c^2}{2c^3}(1-e)^2.\,\chi - \frac{1-c^2}{2c^3}(1-e)^2\,\frac{\text{tang}\,\chi}{1+\text{tang}\,\chi^2}$$

and from this, the coefficients properly simplified with

$$c^2 = \frac{1-e}{1+e} \quad\text{and}\quad e = \sin\kappa$$

results in equation (6) in the text.

and precession]; length of anomalistic year $365^d.25948$ M. T.
$e = \sin \varkappa = 0.016747$, annual decrease in 7th decimal;

$$de = -4.244$$

$\oplus$ in perihelion 1901, I, $0 + 0^d.80758$ Greenwich M. T.

For equation (4) the area covered by the Earth's radius vector during one mean solar day is

$$\Delta S = \frac{\pi . \cos (57' 34''.48)}{365.25636} \quad \text{or} \quad \log \Delta S = 7.9344912 - 10 .$$

Find the Sun's longitude for 1906, March 18, 0^h Greenwich M. T. The Earth passed through its perihelion in 1906, 5 anomalistic years after its passage in 1901, consequently 1906, I, 0, $+ 0^\circ.10498$ up to March 18, 0^h have elapsed, 74^d, 89502 M. T., to which corresponds $\Delta S = 0.644086$, for which we interpolate from our table $\theta = 75^\circ\ 40'\ 14''.6$.

The longitude of perihelion 1906, I, 0 is $101^\circ\ 19'\ 8''.5$ consequently the Sun's longitude March 18, 0^h

$$X = 180^\circ + 75^\circ\ 40'\ 14''.6 + 101^\circ\ 19'\ 8''.5 = 356^\circ\ 59'\ 23''.1$$

This calculation however is to be regarded only as an example to demonstrate the simplicity of my method, compared with that one, expressed in equations (I). The result should

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The deducement of the formula of reduction, used above is:
Differentiating

$$U = \frac{x^m}{X^n}$$

results

$$dU = \frac{m x^{m-1} dx}{X^n} - \frac{n x^m\, dX}{X^{n+1}}$$

consequently for

$$X = a + 2\, bx + cx^2$$

and

$$dX = (2\, b + 2\, cx)\, dx$$

$$dU = \frac{m x^{m-1} dx}{X^n} - \frac{2\, bn\, x^m\, dx}{X^{n+1}} - \frac{2\, cn\, X^{m+1} dx}{X^{n+1}}$$

from which by integration results–

$$\int \frac{x^{m+1} dx}{X^{n+1}} = \frac{m}{2\, cn} \cdot \int \frac{x^{m-1} dx}{X^n} - \frac{b}{c} \int \frac{x^m dx}{X^{n+1}} - \frac{x^m}{2\, cn\, X^n} .$$

Substituting

$$m + 1 = p, \qquad n + 1 = q$$

results formula above.

be corrected for planetary perturbations etc., the same as if derived by the other method.*

Having determined θ by interpolation, the ratio $\frac{\rho}{a}$ an be deduced also with far greater ease from the polar equation of the ellipse:

$$\frac{\rho}{a} = \frac{1 - e^2}{1 + e \cos \theta} \qquad \text{(E)}$$

than from the series equation (I).
Making

$$e \cos \theta = \sin \kappa \cos \theta = \text{tang } \mu^2$$

or $$\text{tang } \mu = \sqrt{\sin \kappa \cos \theta}$$

results from (E)

$$\frac{\rho}{a} = \cos \kappa^2 \cos \mu^2. \qquad (9)$$

This formula (9) fails on account of the negative cosine for $90° < \theta < 270°$. For those values we must substitute

$$\text{tang } \mu^2 = - \sin \kappa \cos \theta$$

which then is positive and the result is

$$\frac{\rho}{a} = \frac{\cos \kappa^2}{1 - \text{tg } \mu^2} = \frac{\cos \kappa^2 \cos \mu^2}{\cos 2\mu} . \qquad (10)$$

It is evident, that as $\frac{\rho}{a}$ is only dependent on e and θ, it may be calculated in a table as a function of θ and such a table for interpolation will remain good for as long as the tables for θ itself.

For computing the ephemeris of any planet, the polar coördinates, obtained as shown above, must be transposed upon the plane of the ecliptic and for that purpose must be given: the (heliocentric) longitude of the planet's ascending node to the perihelion, the arc distance from the ascending node to the perihelion measured in the orbit,—the anomaly of the ascending node,— and the dihedral angle intercepted by the planes of the planet's orbit and the ecliptic. With these data given it is a simple problem of spherical trigonometry to compute in the spherical right triangle, formed by the plane of the ecliptic, that of the planet's orbit and

* Neither the data for Epoch 1901 I, 0, nor the calculatory correctness of the tables can be guaranteed.

one passed through the planet and the poles of the ecliptic, both sides of the right angle, which are: the difference of the planets heliocentric longitude and the longitude of its ascending node and the slope of its radius vector against the plane of the ecliptic.

By multiplying the radius vector with sine and cosine of its slope against the ecliptic, the projection of the planet upon the plane of the ecliptic is performed, and this done the computing of the planet's geocentric longitude and latitude becomes a task of plane trigonometry.

The formulæ for this calculation become simplest by referring the entire system to a system of rectangular coördinates, the plane of the ecliptic for the plane XY and the axis of X through the vernal equinox.

Let x, y and z be the coördinates of the planet, unit of lengths of course the semimajor axis of the Earth's orbit, the coördinates of Earth to be x_1 and y_1, then

$$\text{tang}\,\lambda = \frac{y - y_1}{x - x_1}$$

the projection of the distance between Earth and the planet, the curtate distance

$$D = \pm \frac{x - x_1}{\cos\lambda}$$

and

$$\text{tang}\,\beta = \frac{z}{D}.$$

The true distance required for computing the parallax is

$$\Delta = \frac{D}{\cos\beta}.$$

Allegheny, Pennsylvania.

ASTRONOMICAL PHENOMENA IN 1908.

Eclipses in 1908.

In the year 1908 there will be three eclipses of the Sun and a Lunar Appulse.

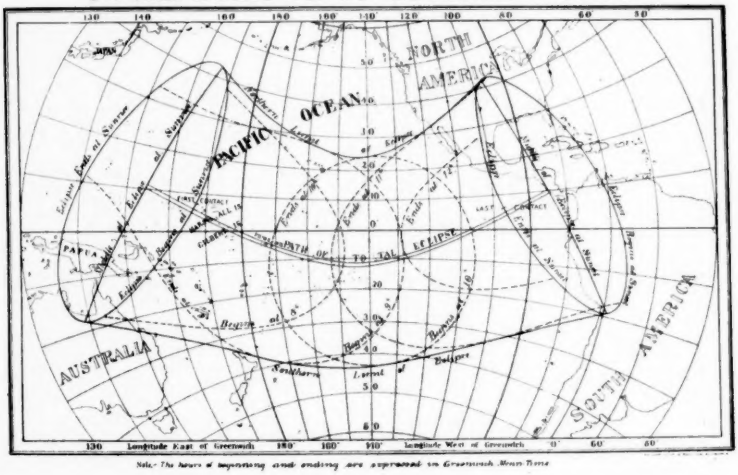
I.—*A Total Eclipse of the Sun*, 1908, January 3, invisible at Washington.

ELEMENTS OF THE ECLIPSE.

Greenwich mean time of ☌ in right ascension, January 3 d 9 h 45 m 14.3 s

Sun and Moon's R. A.	18 h 52 m 47.55 s	Hourly motions	11.02 and 164.96
Sun's declination	22 ° 53 ′ 44.5 ″ S.	Hourly motion	0 ′ 13.9 ″ N.
Moon's declination	22 41 57 1 S.	Hourly motion	0 3.3 S.
Sun's equa. hor. parallax	8.9	Sun's true semidiameter	16 16.0
Moon's equa. hor. parallax	61 15.7	Moon's true semidiameter	16 40.8

TOTAL ECLIPSE OF JANUARY 3, 1908.



Note.—The hours of beginning and ending are expressed in Greenwich Mean Time.

CIRCUMSTANCES OF THE ECLIPSE.

		Greenwich Mean Time. d	h	m	Longitude from Greenwich. °	′		Latitude. °	′	
Eclipse begins	January	3	7	7.7	167	10.6	E.	7	8.3	N.
Central eclipse begins		3	8	4.1	154	39.2	E.	10	45.1	N.
Central eclipse at noon		3	9	45.2	145	13.9	W.	10	51.0	S.
Central eclipse ends		3	11	26.7	84	49.1	W.	9	53.4	N.
Eclipse ends		3	12	23.1	97	20.6	W.	6	16.7	N.

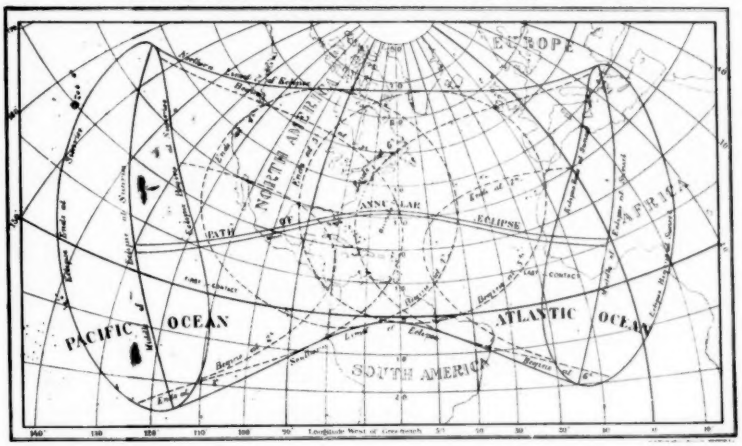
II.—*An Annular Eclipse of the Sun*, 1908, June 28, visible at Washington as a partial eclipse.

ELEMENTS OF THE ECLIPSE.

Greenwich mean time of ☌ in right ascension, June 28 4 30 39.4

Sun and Moon's R. A.	6 28 25.85		Hourly motions	10.37	and	133.40
Sun's declination	23 17 24.0 N.		Hourly motion	0		7.0 S.
Moon's declination	23 25 0.3 N.		Hourly motion	1		19.0 N.
Sun's equa. hor. parallax	8.7		Sun's true semidiameter	15		43.8
Moon's equa. hor. parallax	54 53.7		Moon's true semidiameter	14		56.8

ANNULAR ECLIPSE OF JUNE 28, 1908.



CIRCUMSTANCES OF THE ECLIPSE.

	Greenwich Mean Time.			Longitude from Greenwich.			Latitude.		
	d	h	m	°	′		°	′	
Eclipse begins	June 28	1	29.2	112	24.8	W.	1	58.4	N.
Central eclipse begins	28	2	34.7	129	56.8	W.	4	39.3	N.
Central eclipse at noon	28	4	30.7	66	55.3	W.	31	27.3	N.
Central eclipse ends	28	6	24.9	1	8.0	W.	10	1.0	N.
Eclipse ends	28	7	30.5	18	42.4	W.	7	21.0	N.

III.—*A Lunar Appulse*, December 7, 1908, the Moon just rising at Washington, and visible generally throughout Europe, Asia, Africa, and the extreme eastern portions of North and South America.

ELEMENTS OF THE APPULSE.

Greenwich mean time of ☍ in right ascension, December 7 9 31 32.0

Sun's right ascension	16 56 39.50		Hourly motion			10.94
Moon's right ascension	4 56 39.50		Hourly motion			139.80
Sun's declination	22 39 9.6 S.		Hourly motion	0		16.5 S.
Moon's declination	21 40 28.1 N.		Hourly motion	6		33.6 N.
Sun's equa. hor. parallax	8.9		Sun's true semidiameter	16		14.4
Moon's equa. hor. parrallax	57 5.7		Moon's true semidiameter	15		32.7

CIRCUMSTANCES OF THE APPULSE.

	d	h	m
Nearest approach of Moon to Earth's shadow, December	7	9	55
Computed least distance of Moon's limb from shadow			12″

Angle of position of point of nearest approach, 12° to the west from north point.

IV.—*A Central Eclipse of the Sun*, 1908, December 22-23, invisible at Washington. The central eclipse will be annular at the beginning and end, and will be total in the middle.

ELEMENTS OF THE ECLIPSE.

Greenwich mean time of ☌ in right ascension, December							d	h	m	s
							22	23	49	17.8
	h	m	s				s			
Sun and Moon's R. A.	18	5	36.19	Hourly motions			11.11	and	154.08	
	°	′	″						′	″
Sun's declination	23	26	42.1 S.	Hourly motion			0		1.5 N.	
Moon's declination	23	56	10.4 S.	Hourly motion			2		53.7 S.	
Sun's equa. hor. parallax			8.9	Sun's true semidiameter			16		15.7	
Moon's equa. hor. parallax		58	58.7	Moon's true semidiameter			16		3.5	

CIRCUMSTANCES OF THE ECLIPSE.

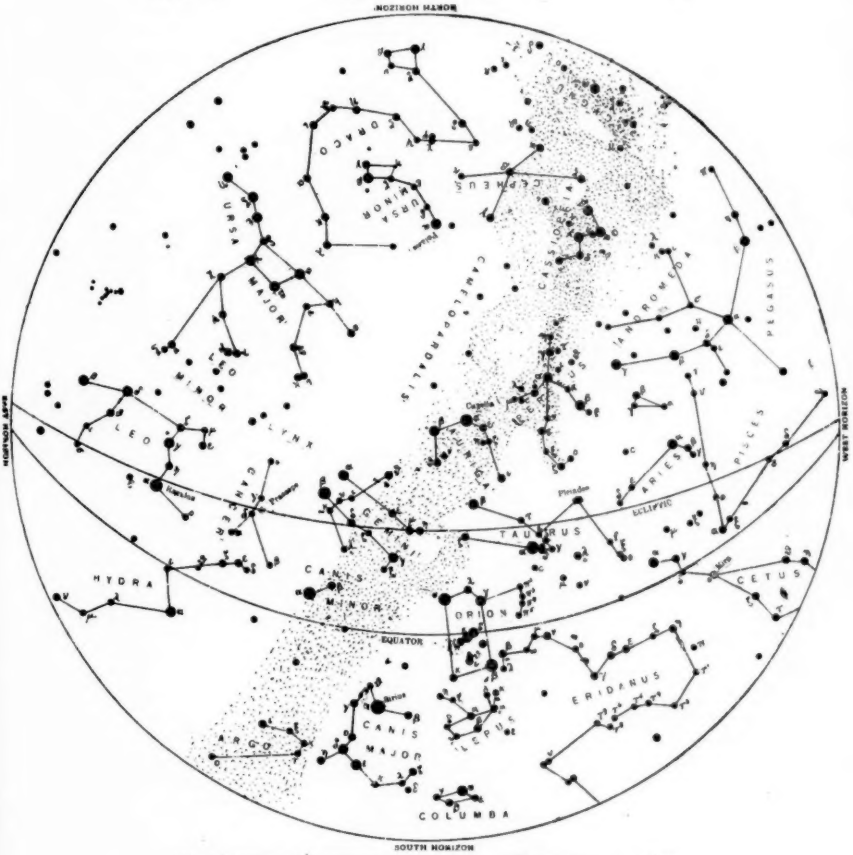
	Greenwich Mean Time.			Longitude from Greenwich.			Latitude.		
	d	h	m	°	′		°	′	
Eclipse begins December	22	21	6.6	52	24.8	W.	12	33.8	S.
Central eclipse begins	22	22	11.3	73	31.7	W.	22	46.1	S.
Central eclipse changes from annular to total	22	22	57.9	28	3.0	W.	46	13.9	S.
Central eclipse at noon	22	23	49.3	2	27.6	E.	53	46.0	S.
Central eclipse changes from total to annular	23	0	37.9	36	18.0	E.	51	24.0	S.
Central eclipse ends	23	1	17.7	86	1.9	E.	31	54.4	S.
Eclipse ends	23	2	22.3	64	14.0	E.	21	52.3	S.

The regions within which the first and second eclipses of the Sun are visible are laid down on the accompanying charts, from which, by means of the dotted lines, the Greenwich times of beginning and ending at any place may be found with an uncertainty which will vary from three or four minutes for a high Sun, to fifteen or twenty minutes when the Sun is near the horizon.

The Planets.

The apparent courses of the planets among the stars during the year 1908 are shown upon the charts, Figures 3 and 4. *Mercury* describes a course quite similar to that of last year, beginning in Sagittarius and running outward to the constellation Aquarius, where in February and March the planet in circling round the Sun will make a large closed loop in its path. Then it will move eastward again until, in June and July, another large closed loop will be formed in the constellation Gemini. Again in October and November the planet retrogrades in movement, forming this time an open Z-shaped loop in its apparent path. Mercury will be at inferior conjunction with the Sun February 28, July 4 and October 28. The following table shows the condition of Mercury as to brightness and distance from the Sun at the several greatest elongations which will occur this year. It will appear from this together with the declination of Mercury as shown on the chart, that the planet will be most easily seen about February 13.

Date.	Elongation.	Distance.	Brightness.
Feb. 13	East	18° 09′	65
Mar. 27	West	27 49	33
June 7	East	23 59	34
July 25	West	19 51	43
Oct. 4	East	25 34	35
Nov. 13	West	19 19	58



THE CONSTELLATIONS AT 9:00 P. M., FEBRUARY 1, 1908.

Venus begins the year in Capricorn and moves eastward along the Zodiac until June, when she makes a large loop in Gemini. The dotted curve on the chart Figure 3 shows the apparent course of Venus for the year. Venus and Mercury will be at inferior conjunction with the Sun at very nearly the same time in July, the conjunction of Mercury July 4 and that of Venus July 5. Venus will be evening star during the first half of the year, increasing in brilliancy until May 30, then decreasing rapidly until the conjunction. After that she will be morning star increasing very rapidly in brightness and remaining very brilliant for the rest of the year.

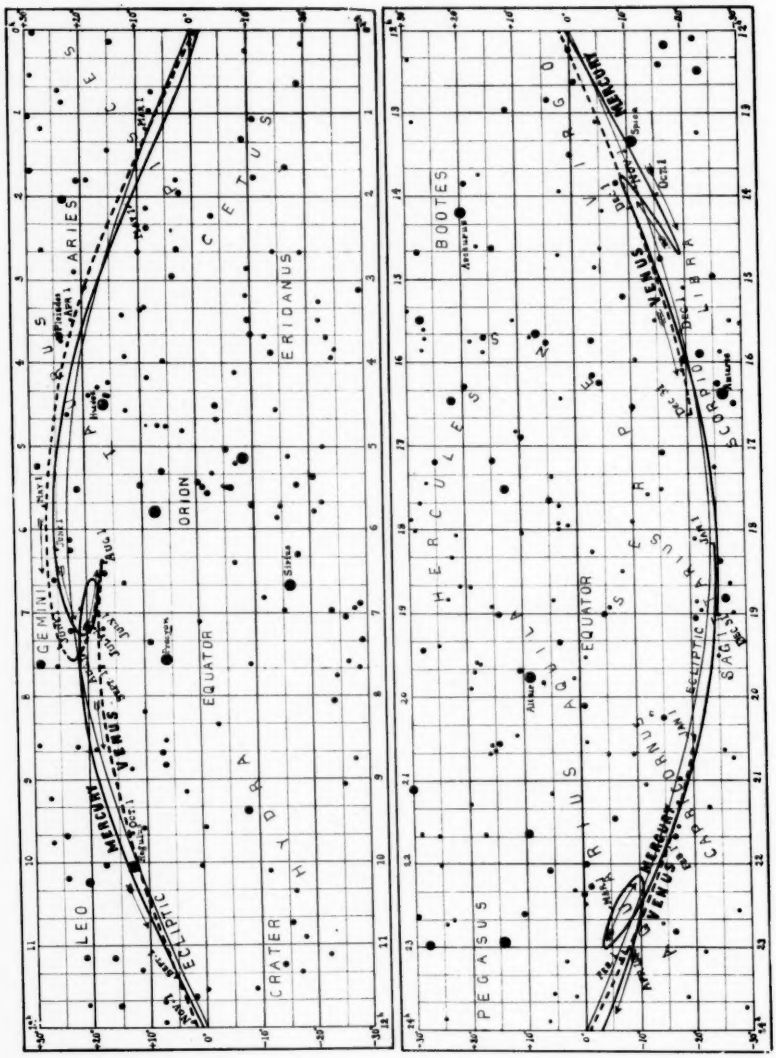


FIG. 3. The Apparent Paths of the Planets Mercury and Venus during the Year 1908.

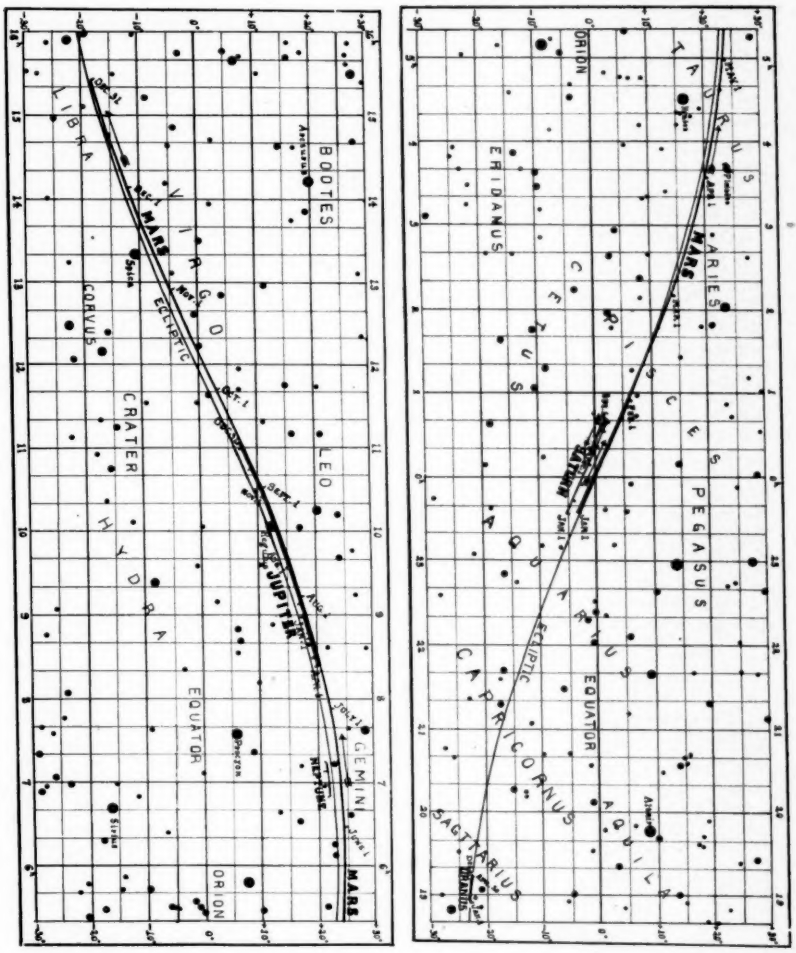


FIG. 4. Apparent Paths of the Planets Mars, Jupiter, Saturn, Uranus and Neptune during the Year 1908.

The path of Mars this year forms no loop, but extends smoothly along the ecliptic from Aquarius to Libra.

Jupiter's course is retrograde in Cancer for the first three months and after that is direct for the rest of the year, extending through Cancer and the greater part of Leo. The planet will be in conjunction with the first magnitude star Regulus on the night of September 4 and will then be only 22′ north of the star.

Saturn's path lies along the boundary between Aquarius, Pisces and Cetus and the motion is direct until the latter part of July, then retrograde until December 1. The angle between the line of sight from Earth to Saturn and the plane of the rings will increase to nearly 8° in July, then decrease to 5° in December.

The path of *Uranus* is a short one in Sagittarius, the movement being direct until April 20, then retrograde until September 21 and direct for the rest of the year.

Neptune describes a very short course in Gemini, retrograding until March 22 and again after October 20.

Phases of the Moon.

Washington Mean Time.

1908	New Moon h	m	First Quarter h	m	Full Moon h	m	Last Quarter h	m
Jan.	3 4	35.1	Jan. 9 20	44.6	Jan. 17 20	28.6	Jan. 25 21	53 0
Feb.	1 15	28.2	Feb. 8 11	19.2	Feb. 16 15	57.1	Feb. 24 10	16.0
Mar.	2 1	48.6	Mar. 9 4	33.8	Mar. 17 9	20.2	Mar. 24 19	23.3
Mar.	31 11	53.9	Apr. 7 23	23.2	Apr. 15 23	47.0	Apr. 23 1	58.4
Apr.	29 22	24.8	May 7 18	15.0	May 15 11	24.1	May 22 7	08.9
May	29 10	06.2	June 6 11	47.8	June 13 20	46.9	June 20 12	17.8
June	27 23	23.2	July 6 03	16.7	July 13 4	39.6	July 19 18	53.4
July	27 14	08.5	Aug. 4 16	32.1	Aug. 11 11	50.5	Aug. 18 4	17.2
Aug.	26 5	50.6	Sept. 3 3	42.5	Sept. 9 19	15.0	Sept. 16 17	25.1
Sept.	24 21	51.1	Oct. 2 13	05.4	Oct. 9 3	55.1	Oct. 16 10	27.1
Oct.	24 13	38.3	Oct. 31 21	08.0	Nov. 7 14	49.7	Nov. 15 6	32.8
Nov.	23 4	44.8	Nov. 30 4	36.1	Dec. 7 4	35.8	Dec. 15 4	04.2
Dec.	22 18	41.4	Dec. 29 12	31.6				

Occultations visible at Washington.

The list of occultations given by the *American Ephemeris* as visible at Washington during this year numbers 101. We plan to give the list for each month in POPULAR ASTRONOMY one month ahead of time so that our readers may have ample notice of each. The list for February is as follows:

Date 1908	Star's Name	Magnitude.	IMMERSION. Washington M.T. h	m	Angle f'm N. °	EMERSION. Washington M.T. h	m	Angle f'm N °	Duration. h	m
Feb. 10	BD.+19° 811	6.2	11	21	124	12	16	224	0	55
11	ζ Tauri	3.0	5	04	77	6	27	249	1	23
13	δ Geminorum	3.5	4	38	107	5	42	240	1	04
14	Piazzi viii, 42	6.0	10	32	126	11	59	262	1	27
24	B A. C. 5567	6.5	18	41	144	19	48	250	1	07
25	52 Ophiuchi	6.4	16	06	38	16	33	355	0	27
26	Bradley 2335	5.8	16	37	109	17	47	270	1	10
26	26 Sagittarii	6.1	18	59	182	19	04	190	0	05

Comets.

The periodic comets which were due at perihelion in 1907 have thus far none of them been detected. In 1908 four are due, two which have

been observed at more than one apparition and two which have been seen but once.

1. The first is Encke's comet which reappears every three and a third years. It is due at perihelion April 30 but will be in best position to be observed in June and July. It will be most easily seen by observers on the Southern Hemisphere.

2. The comet Tempel-Swift will be at perihelion towards the last of September. It will be in favorable position for observation during the autumn months.

3. Brook's comet 1886 IV comes to perihelion in October, when it will be almost behind the Sun and so is not likely to be detected.

4. The comet Denning 1894 I is due at perihelion about the middle of December under fairly favorable conditions. It is most likely to be found in the early part of the winter.

Variable Stars.

The list of variable stars is increasing so rapidly now that the task of predicting the times of minima and maxima is becoming a laborious one. For 1908 we expect to continue the plan of former years of giving the times of all the minima of the Algol type stars, except where the period is less than one day. These predictions are given only to the nearest hour of Greenwich mean time, but the calculations have been carried out to the nearest minute. The computations have been checked independently at the beginning and the end of the year, as well as by a subsidiary check at every tenth minimum, so that we hope that the errors are pretty thoroughly eliminated. For the stars of short period not of the Algol type the times of maxima have been calculated in the same way. For stars whose periods are less than half a day it seems hardly worth while to prepare tables, since observations of these stars will be valuable at any hour.

We are endeavoring to keep our list of elements of short period variables up to date and shall be glad if any observer will send us corrections or new elements which he does not see noted in our monthly notes.

Meteor Showers.

The following is a list of the radiant points of the most brilliant showers in Mr. Denning's list given in the *Companion to the Observatory* for 1907.

Date	Radiant α	Radiant δ	Remarks
Jan. 2-3	250°	+53°	Swift; long paths
Apr. 20-22	271	+33	Swift.
May 1-6	338	− 2	Swift; streaks.
July 25-31	339	−11	Slow; long.
Aug. 10-12	45	+57	Swift; streaks.
Oct. 18-20	92	+15	Swift; streaks.
Nov. 14-16	150	+22	Swift; streaks.
Nov. 17-23	25	+43	Very slow; trains.
Dec. 10-12	108	+33	Swift; short.

The Perseids, with maximum on August 11, are visible for a considerable period and their radiant point exhibits a motion to E. N. E. among the stars of about 1° per day. Its position for July 19, is about $\alpha = 19°$, $\delta = +50°$ and for August 16 about $\alpha = 53°$ $\delta = +58°$.

PHENOMENA OF JUPITER'S SATELLITES.

Central Standard Time, reckoning from noon.

		h	m			
Feb.	1	5	35	I	Tr.	In.
		5	39	I	Sh.	In.
		7	55	I	Tr.	Eg.
		7	59	I	Sh.	Eg.
		17	30	III	Oc.	Dis.
	2	5	04	I	Ec.	Dis.
		14	04	II	Oc.	Dis.
		17	12	II	Ec.	Re.
	4	8	53	II	Tr.	In.
		8	28	II	Sh.	In
		11	05	II	Tr.	Eg.
		11	24	II	Sh.	Eg.
	5	5	13	IV	Tr.	Eg.
		6	49	IV	Sh.	Eg.
		7	19	III	Tr.	In.
		8	03	III	Sh.	In.
		11	01	III	Tr.	Eg.
		11	45	III	Sh.	Eg.
		15	33	I	Oc.	Dis.
	6	6	29	II	Ec.	Re.
		12	52	I	Tr.	In.
		13	05	I	Sh.	In.
		15	12	I	Tr.	Eg.
		15	25	I	Sh.	Eg.
	7	9	59	I	Oc.	Dis.
		12	31	I	Ec.	Re.
	8	7	18	I	Tr.	In.
		7	34	I	Sh.	In.
		9	38	I	Tr.	Eg.
		9	54	I	Sh.	Eg.
	9	7	00	I	Ec.	Re.
		16	18	II	Oc.	Dis.
	11	10	23	II	Tr.	In.
		11	04	II	Sh.	In.
		13	19	II	Tr.	Eg.
		14	00	II	Sh.	Eg.
	12	10	35	III	Tr.	In.
		12	02	III	Sh.	In.
		14	17	III	Tr.	Eg.
		15	44	III	Sh.	Eg.
	13	5	26	II	Oc.	Dis.
		7	58	IV	Oc.	Dis.
		9	04	II	Ec.	Re.
		14	36	I	Tr.	In.
		14	59	I	Sh.	In.
		16	21	IV	Ec.	Re.
		16	56	I	Tr.	Eg.
		17	19	I	Sh.	Eg.

		h	m			
Feb.	14	11	43	I	Oc.	Dis.
		14	25	I	Ec.	Re.
	15	9	02	I	Tr.	In.
		9	28	I	Sh.	In.
		11	23	I	Tr.	Eg.
		11	48	I	Sh.	Eg.
	16	5	21	III	Ec.	Re.
		6	09	I	Oc.	Dis.
		8	54	I	Ec.	Re.
	17	5	49	I	Tr.	Eg.
		6	17	I	Sh.	Eg.
	18	12	39	II	Tr.	In.
		13	40	II	Sh.	In.
		15	35	II	Tr.	Eg.
		16	36	II	Sh.	Eg.
	19	13	54	III	Tr.	In.
		16	01	III	Sh.	In.
	20	7	42	II	Oc.	Dis.
		11	40	II	Ec.	Re.
		16	21	I	Tr.	In.
		16	54	I	Sh.	In.
	21	13	28	I	Oc.	Dis.
		14	44	IV	Tr.	In.
		16	20	I	Ec.	Re.
	22	5	54	II	Sh.	Eg.
		10	47	I	Tr.	In.
		11	22	I	Sh.	In.
		13	00	I	Tr.	Eg.
		13	43	I	Sh.	Eg.
	23	7	55	I	Oc.	Dis.
		9	28	III	Ec.	Re.
		10	49	I	Ec	Re.
	24	5	51	I	Sh.	In.
		7	34	I	Tr.	Eg.
		8	11	I	Sh.	Eg.
	25	14	57	II	Tr.	In.
		16	16	II	Sh.	In.
	27	9	59	II	Oc.	Dis.
		14	15	II	Ec.	Re.
	28	15	14	I	Oc.	Dis.
		18	15	I	Ec.	Re.
	29	5	34	II	Sh.	In.
		7	03	II	Tr.	Eg.
		8	31	II	Sh.	Eg.
		12	33	I	Tr.	In.
		13	17	I	Sh.	In.
		14	53	I	Tr.	Eg.
		15	37	I	Sh.	Eg.

Note.—In., denotes ingress; Eg., denotes egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., Transit of the Satellite: Sh., transit of the shadow.

VARIABLE STARS.

The Variable 136.1907 Andromedæ.—In A.N. 4215 Mr. A. A. Nijland gives observations of this new variable, while together with the previous observations of Blajko, of Moscow, and von Biesbroeck, of Uccle, indicate

that the period is perhaps 34.67 days. The fractions one-half, one-third, one-fourth and perhaps one-eighth of this period are possible, but smaller fractions are excluded by Nijland's observations. The three minima observed by the above mentioned observers were:

1907	Aug.	8	8.7^h to 11.5^h	Blajko.
	Sept.	12	8.0 to 16.1	von Biesbroeck and Nijland.
	Oct.	17	5.6 to 11.2	von Biesbroeck and Nijland.

The duration of the minimum is not less than 9 hours nor more than 24 hours.

New Variables 141 and 142.1907.—These are announced in *A. N.* 4213 by Professor W. Ceraski and were discovered by Mme. L. Ceraski on the Moscow photographs. Their positions are

	α 1855	δ 1855	α 1900	δ 1900
	h m s	° ′	h m s	° ′
141.1907 Andromedae	22 52 54.	+ 42 04.	22 54 55	+ 42 18.
142.1907 Cassiopeiae	0 03 08.3	+ 54 05.1	0 05 28.1	+ 54 20.1

The first is probably of long period and on fourteen photographs taken in the years 1900-07 ranges from 9.6 to <11 magnitude.

The second is probably of the Algol type or perhaps irregular. It is the star BD + 54° 7, noted in the BD. as of magnitude 8.3. On 34 photographs taken in the interval 1895-1907 it appears of almost constant brightness, 8^m.7 to 8.^{m}9 but upon two of the dates September 18, 1905 and August 13, 1907 its magnitude is estimated at 9.2 and 9.4.

New Variables 143 and 144.1907.—Two more new variables are announced by Professor Ceraski in *A. N.* 4215. They were discovered on the Moscow photographs by Mme. L. Ceraski, and their positions are

	α 1855	δ 1855	α 1900	δ 1900
	h m s	° ′	h m s	° ′
143.1907 Andromedae	23 06 40	+ 45 21	23 08 44	+ 45 36
144.1907 Cassiopeiae	0 07 26.1	+ 57 37.2	0 09 47.9	+ 57 52.2

The first is probably of the Algol type, ranging from about 10.5 to 11.2 magnitude. The second is of short period and varies between 9.2 and 10.0 magnitude. It is star BD + 57° 42.

Sixteen New Variable Stars.—In the Circular No. 132 of the Harvard College Observatory, is a list of 23 stars having peculiar spectra, sixteen of which are noted as variable. The Variable Star Committee of the Astronomische Gesellschaft have given these the numbers 147 to 162.1907.

No.	Constellation	DM	R.A. 1900	Dec. 1900	Mag.
		°	h m	° ′	
147.1907	Arietes	+11 305	2 09.6	+11 46	8.2 - 8.8
148.1907	Aurigae	+45 1324	6 28.2	+45 43	8.5 - 9.4
149.1907	Monocerotis	− 4 1708	6 48.3	− 4 27	9.2 - 10.4
150.1907	Canis Major	−22 1850	7 19.4	−22 47	11.0 - 11.9
151.1907	Cancri	+15 1808	8 16.8	+15 19	9.4 - 10.3
	Ursa Major	...	9 44.4	+53 07	...
	Vela	−49 5234	10 21.0	−49 54	9.6
152.1907	Carinae	−72 1048	10 48.7	−72 14	9.8 - 11.5
153.1987	Muscae	R	11 35.0	−72 00	8.6 - 10 0
154.1907	Virginis	...	12 00.0	+12 56	8.5 - 12.5
155.1907	Muscae	R	12 17.4	−74 57	8.8 - 10.6
156.1907	Corvi	−16 3503	12 32.3	−16 43	8.8 - 10.5

No.	Constellation	DM.	R.A. 1900 h m	Dec. 1900 ° ′	Mag.
157.1907	Centauri	−63 2720	13 15.5	−63 42	9.0 - 10.5
	Centaurus	−56 5891	13 36.4	−56 16	6.8
	Circinus	−67 2622	14 30.9	−67 46	7.0
158.1907	Coronae Bor.	+39 2901	15 37.8	+38 53	7.0 - 8.3
159.1907	Draconis	+57 1786	17 35.4	+57 48	8.0 - 9.7
	Ophiuchus	+ 6 3898	18 37.1	+ 6 43	9.0
	Aquila	...	20 07.1	+11 35	...
160.1907	Draconis	+74 861	20 25.9	+64 56	8.3 - 10.5
161.1907	Cygni	+32 3850	20 27.6	+32 14	8.5 - 9.5
162.1907	Pegasi	+34 4597	22 01.4	+34 52	8.2 - 9.2
	Aquarius	−21 6376	23 06.3	−21 32	9.0

Fifteen New Variable Stars.—Circular No. 133 of the Harvard College Observatory contains a list of fifteen more new variables found by Miss Leavitt in examining the chart plates for bright variable stars. These have been given the numbers 163 to 177.1906 by the Variable Star Committee of the Astronomische Gesellschaft.

No.	Constellation	DM.	R.A. 1900 h m s	Dec. 1900 ° ′	Mag.
163.1907	Hydrae	− 7 2715	9 00 48	− 7 51.5	9.0 - 11.5
164.1907	Hydrae	...	8 19 53	− 6 21.8	10.0 -<11.5
165.1907	Leonis	+26 1981	9 31 05	+26 41.4	9.0 - 10.6
166.1907	Leo. Minor	...	9 42 33	+33 45.2	9.5 - 11.5
167.1907	Leonis	+27 1818	9 46 25	+27 22.3	9.8 - 10.4
168.1907	Sextantis	+ 2 2264	9 48 16	+ 2 31.6	8.9 - 9.6
169.1907	Leo. Minor	...	10 00 07	+39 51.4	11.0 - 11.8
170.1907	Leonis	+24 2183	10 02 08	+24 28.9	9.0 - 9.8
171.1907	Hydrae	− 9 3017	10 07 24	− 9 49.5	9.1 . 9.8
172.1907	Bootes	...	15 26 47	+36 08.1	10 2 - 11.0
173.1907	Coron. Cor.	+36 2672	15 54 47	+36 17.9	9.6 - 10.6
174.1907	Herculis	+25 3031	16 03 14	+25 10.3	8.8 - 9.7
175 1907	Herculis	+38 2803	16 32 28	+28 10.1	8.4 - 9.7
176.1907	Herculis	+17 3117	16 49 54	+17 00.0	10.0 - 10.8
177.1907	Herculis	+22 3048	16 57 15	+22 36.7	10.0 -<11.0

Nos. 163, 165 and 166 appear to be of the Algol type.

The Variable Star Z Ceti.—In *A. N.* 4215 Mr. Wilhelm Luther gives the results of his observations in 1907 of this long period variable, and finds from these combined with earlier observations the formula

$$\text{Maximum} = 2417838 + 185^{d}.8\ \text{E.}$$

Error in Predicted Minima of Algol.—In computing the minima of Algol for 1907 our computer managed to make an error of one day in both the direct computation and the check, which makes the dates all one day too large after March 13. The computations for 1908 have all been subjected to a second independent check at the end of the year as well as an intermediate check every tenth minimum, so that we hope that no errors remain undetected.

Observations of Nine Algol Type Variables.—In *A.N.* 4211 A. A. Nijland of Utrecht, finds that the error of the ephemeris of ALGOL according to Chandler's elements, from the average of four observed minima in 1907 is 36^{m}. The minima occur 36^{m} earlier than predicted in our tables.

U SAGITTÆ.—Mr. Nijland gives the following corrected elements of U Sagittæ:

$$\text{Minimum} = 2415690.276 + 3.^{d}380603\ \text{E.}$$

According to these the minima should occur about an hour earlier than the times given in our tables. The entire duration of minimum is about 11 hours; the star remains stationary at minimum for two hours, at magnitude 9.2 while at full brightness it is 6.85^m.

Z PERSEI.—Mr. Nijland also finds the observations of this star to be departing from the ephemerides, the minimum coming $2^h\ 27^m$ earlier than predicted on July 31, 1907. From the comparison of nine minima in 1905-07 he finds the following corrected elements:

$$\text{Minimum} = 2416009.693 + 3.056463\ \text{E.}$$

According to these elements the correction to our tables for January will be $-2^h\ 30^m$. The light change occupies about 9 hours; the star is stationary at minimum for $2^h.4$, the magnitude being 12.4 while its normal brightness is 9.6.

Y CAMELOPARDI.—For this star also the correction is approximately $-1^h\ 15^m$ and the new elements according to Mr. Nijland are

$$\text{Minimum} = 2417615.382 + 3^d.305461\ \text{E.}$$

Our tables of this star for February have been corrected. The duration of the light change is about 12 hours and the range of brightness from 9.7^m to $11^m.8$.

Z DRACONIS.—The corrected period given by Nijland is printed as $1^d.374145$, but this must be in error and probably should be $1^d.3574145$, the new formula becoming

$$\text{Minimum} = 2416177.399 + 1^d.3574145\ \text{E.}$$

The correction to our tables for 1908 is about -1^h. The minimum is very sharp, the entire duration of the light change being only 5 hours, and the range of brightness from $10^m.1$ to $12^m.3$.

RW GEMINORUM.—The observations of this star by Nijland do not indicate any certain correction to the elements used. The minimum lasts about 12 hours and the magnitude is 12.1 at minimum and 9.75 at normal.

RZ CASSIOPELÆ.—For this star Nijland finds the following formula to satisfy the observations up to August 27, 1907;

$$\text{Minimum} = 2417649.455 + 1^d.19526\ \text{E.}$$

This makes the error of the table for January about 3^h. The minima occur three hours later than the times given in the table. The duration of the change of light at minimum is about 5.4^h and the range of magnitude from 6.5 to 8.1 magnitude.

RR DELPHINI.—This star is also departing widely from the ephemerides, the correction being now about -4^h. Mr. Nijland finds for the corrected formula

$$\text{Minimum} = 2417424.514 + 4^d.5993\ \text{E.}$$

The normal magnitude is 10.5 and at minimum it descends to 11.8. The minimum lasts about 14 hours.

RY PERSEI.—From his own observations together with those of Mr. Ichinohe (A. N. 4172), Nijland finds these corrected elements:

$$\text{Minimum} = 2417523.475 + 6^d.8640\ \text{E.}$$

The minimum lasts about 23 hours and the range of magnitude is from 8.1 to 10.6. The times in our table for this star for January, 1908, should be diminished by 2 hours.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours.]

U Cephei

	d	h
Feb.	2	0
	4	12
	7	0
	9	12
	12	0
	14	11
	16	23
	19	11
	21	23
	24	11
	26	23
	29	10

Z Persei

	d	h
Feb.	3	0
	6	2
	9	3
	12	5
	15	6
	18	7
	21	9
	24	10
	27	11

RY Persei

	d	h
Feb.	4	12
	11	9
	18	5
	25	2

RZ Cassiop.

	d	h
Feb.	1	9
	2	14
	3	18
	5	0
	6	4
	7	8
	8	13
	9	18
	10	22
	12	3
	13	8
	14	12
	15	17
	16	22
	18	3
	19	7
	20	12
	21	17
	22	21
	24	2
	25	7
	26	11
	27	16
	28	21

RX Cephei

	d	h
Feb.	23	22

Algol

	d	h
Feb.	2	20
	5	17
	8	14
	11	10
	14	7
	17	4
	20	1
	22	22
	25	19
	28	15

RT Persei

	d	h
Feb.	1	8
	2	15
	3	11
	4	8
	5	4
	6	0
	6	21
	7	17
	8	13
	9	10
	10	6
	11	3
	11	23
	12	19
	13	16
	14	12
	15	9
	16	5
	17	1
	17	22
	18	18
	19	15
	20	11
	21	7
	22	4
	23	0
	23	20
	24	17
	25	13
	26	10
	27	6
	28	2
	28	23
	29	19

λ Tauri

	d	h
Feb.	4	19
	8	18
	12	17
	16	15
	20	14
	24	13
	28	12

RW Tauri

	d	h
Feb.	1	17
	4	11
	7	6
	10	0
	12	19
	15	13
	18	18
	21	2
	23	21
	26	15
	29	9

RV Persei

	d	h
Feb.	2	8
	4	7
	6	6
	8	6
	10	5
	12	5
	14	4
	16	3
	18	3
	20	2
	22	1
	24	1
	26	0
	28	23
	29	23

RW Persei

	d	h
Feb.	13	11
	26	16

RS Cephei

	d	h
Feb.	13	1
	25	11

RW Geminorum

	d	h
Feb.	1	3
	4	0
	9	21
	9	18
	12	14
	15	11
	18	9
	21	5
	24	1
	26	2
	29	19

RW Monoc. (24.1907)

	d	h
Feb.	2	7
	4	5
	6	3
	8	1
	9	22
	11	20
	13	18
	15	16
	17	14
	19	11
	21	9
	23	7
	25	5
	27	2
	29	0

RU Monoc.

	d	h
Feb.	1	21
	2	19
	3	17
	4	14
	5	12
	6	9
	7	7
	8	4
	9	2
	9	23
	10	21
	11	18
	12	16
	13	13
	14	11
	15	8
	16	6
	17	3
	18	1
	18	22
	19	20
	20	17
	21	15
	22	12
	23	10
	24	7
	25	5
	26	2
	27	0
	27	21
	28	19
	29	16

R Canis Maj.

	d	h
Feb.	1	1
	2	4
	3	7
	4	11
	5	14
	6	17
	7	20
	9	0
	10	3
	11	6
	12	9
	13	13
	14	16
	15	19
	16	22
	18	2
	19	5
	20	8
	21	12
	22	15
	23	18
	24	21
	26	1
	27	4

R Canis Maj.

	d	h
Feb.	28	7
	29	10

Y Camelopardi

	d	h
Feb.	3	16
	7	0
	10	7
	13	14
	16	22
	20	5
	23	12
	26	20

R Puppis

	d	h
Feb.	1	3
	7	14
	14	0
	20	10
	26	21

V Puppis

	d	h
Feb.	1	19
	3	6
	4	17
	6	4
	7	15
	9	2
	10	13
	11	23
	13	10
	14	21
	16	8
	17	19
	19	6
	20	17
	22	4
	23	15
	25	2
	26	13
	27	23
	29	10

X Carinæ

	d	h
Feb.	1	11
	2	13
	3	15
	4	17
	5	19
	6	21
	7	23
	9	1
	10	3
	11	5
	12	7
	13	9
	14	11
	15	13
	16	15
	17	17
	18	19
	19	21

Minima of Variable Stars of the Algol Type.—Continued.

X Carinæ

	d	h
Feb.	20	23
	22	1
	23	3
	24	5
	25	7
	26	9
	27	11
	28	13
	29	15

S Cancri

Feb.	6	1
	15	12
	25	0

S Velorum

Feb.	4	2
	10	0
	15	23
	21	21
	27	19

RR Velorum

Feb.	1	16
	3	13
	5	9
	7	6
	9	2
	10	23
	12	19
	14	16
	16	12
	18	9
	20	5
	22	2
	23	22
	25	19
	27	15
	29	12

SS Carinae

Feb.	4	0
	7	8
	10	15
	13	22
	17	5
	20	13
	23	20
	27	3

Z Draconis

Feb.	1	6
	2	15
	3	23
	5	8
	6	16
	8	1
	9	10
	10	18
	12	3
	13	11
	14	20
	16	5
	17	13
	18	22

Z Draconis

Feb.	20	6
	21	15
	22	23
	24	8
	25	17
	27	1
	28	10
	29	18

RZ Centauri

Feb.	1	18
	2	16
	3	15
	4	13
	5	12
	6	10
	7	9
	8	8
	9	6
	10	5
	11	3
	12	2
	13	0
	13	23
	14	21
	15	20
	16	18
	17	17
	18	15
	19	14
	20	12
	21	11
	22	9
	23	8
	24	6
	25	5
	26	3
	27	2
	28	0
	28	23

SS Centauri

Feb.	2	9
	4	20
	7	8
	9	19
	12	7
	14	18
	17	6
	19	17
	22	5
	24	16
	27	4
	29	15

δ Libræ

Feb.	2	5
	4	13
	6	21
	9	5
	11	13
	13	21
	16	4

δ Libræ

	d	h
Feb.	18	12
	20	20
	23	4
	25	12
	27	20

U Coronæ

Feb.	1	12
	4	23
	8	10
	11	20
	15	7
	18	18
	22	5
	25	16
	29	3

R Aræ

Feb.	4	20
	9	7
	13	17
	18	3
	22	13
	26	23

U Ophiuchi

Feb.	1	2
	1	22
	2	18
	3	14
	4	10
	5	7
	6	3
	6	23
	7	19
	8	15
	9	11
	10	7
	11	3
	12	0
	12	20
	13	16
	14	12
	15	8
	16	4
	17	0
	17	20
	18	17
	19	13
	20	9
	21	5
	22	1
	22	21
	23	17
	24	14
	25	10
	26	6
	27	2
	27	22
	28	18
	29	14

Z Herculis

	d	h
Feb.	1	12
	3	9
	5	12
	7	9
	9	11
	11	8
	13	11
	15	8
	17	11
	19	8
	21	11
	23	8
	25	11
	27	8
	29	11

RS Sagittarii

Feb.	3	2
	5	11
	7	21
	10	7
	12	17
	15	3
	17	13
	19	23
	22	9
	24	19
	27	5
	29	15

V Serpentis

Feb.	1	7
	4	18
	8	5
	11	16
	15	3
	18	14
	22	1
	25	12
	28	22

RZ Draconis

Feb.	2	1
	3	3
	4	6
	5	8
	6	10
	7	13
	8	15
	9	18
	10	20
	11	23
	13	1
	14	3
	15	6
	16	8
	17	11
	18	13
	19	16
	20	18
	21	21
	22	23
	24	1

RZ Draconis

Feb.	25	4
	26	6
	27	9
	28	11
	29	14

RX Herculis

Feb.	1	9
	2	7
	3	4
	4	1
	4	23
	5	20
	6	17
	7	15
	8	12
	9	10
	10	7
	11	4
	12	2
	12	23
	13	20
	14	18
	15	15
	16	12
	17	10
	18	7
	19	4
	20	2
	20	23
	21	20
	22	18
	23	15
	24	12
	25	10
	26	7
	27	4
	28	2
	28	23
	29	20

SX Sagittarii

Feb.	1	7
	3	8
	5	10
	7	12
	9	14
	11	16
	13	18
	15	20
	17	21
	19	23
	22	1
	24	3
	26	5
	28	7

RR Draconis

Feb.	3	2
	5	22
	8	18
	11	14
	14	10

Minima of Variable Stars of the Algol Type.—Continued.

RR Draconis			U Scuti			RX Draconis			RV Lyræ			WW Cygni		
	d	h		d	h		d	h		d	h		d	h
Feb.	17	6	Feb.	11	16	Feb.	2	0	Feb.	18	8	Feb.	4	15
	20	2		12	15		3	22		21	23		7	23
	22	22		13	14		5	19		25	13		11	6
	25	18		14	13		7	16		29	4		14	14
	28	14		15	12		9	14	U Sagittæ				17	21
				16	10		11	11	Feb.	3	14		21	5
RZ Ophiuchi				17	9		13	9		7	0		14	13
Feb.	11	14		18	8		15	6		10	9		27	20
U Scuti				19	7		17	4		13	18	SW Cygni		
Feb.	1	4		20	6		19	1		17	2	Feb.	2	9
	2	3		21	5		20	23		20	11		6	23
	3	2		22	4		22	20		23	20		11	12
	4	1		23	3		24	18		27	5		16	2
	4	23		24	2		26	15	SY Cygni				20	16
	5	22		25	1		28	13	Feb.	6	5		25	6
	6	21		26	0	RV Lyræ				12	5	VW Cygni		
	7	20		26	23	Feb.	3	23		18	5	Feb.	3	17
	8	19		27	22		7	13		24	5		12	3
	9	18		28	20		11	4	WW Cygni				20	14
	10	17		29	19		14	18	Feb.	1	7		29	0

Maxima of Variable Stars of Short Period not of the Algol Type.

Unless otherwise indicated the times of maxima only are given; and the times of minima may be found by subtracting the interval printed in parentheses under the names of the stars.

RW Cassiop.			RR Geminorum			T Velorum			S Crucis			S Triang. Austr.		
	d	h		d	h		d	h		d	h		d	h
	(−5	19)	Period 9.5h				(−1	10)		(−1	12)	Feb.	16	23
Feb.	8	2		(−0	2)	Feb.	1	7	Feb.	4	21		23	6
	22	22	Feb.	1	9		5	23		9	13		29	14
RX Aurigæ				2	13		10	14		14	6	S Normæ		
	(−4	0)		3	18		15	5		18	22		(−4	10)
Feb.	6	9		4	23		19	21		23	15	Feb.	2	17
	18	0		6	3		24	12		28	7		12	11
	29	15		7	8		29	3	W Virginis				22	5
Y Aurigæ				8	12	W Carinæ				(−8	5)	RW Draconis.		
	(−0	18)		9	17		(−1	0)	Feb.	3	22	Period 10.6h		
Feb.	3	7		10	22	Feb.	4	9		21	5		(−0	3)
	7	4		12	2		8	18	V Centauri			Feb.	1	9
	11	0		13	7		13	3		(−1	11)		2	7
	14	21		14	11		17	12	Feb.	3	18		3	4
	18	18		15	16		21	20		9	6		4	1
	22	14		16	21		26	5		14	18		4	22
	26	11		18	1	S Muscæ				20	6		5	20
T Monoc.				19	6		(−3	11)		25	17		6	17
	(−7	23)		20	10	Feb.	2	14	R Triang. Austr.				7	14
Feb.	8	10		21	15		12	5		(−1	0)		8	11
W Geminorum				22	20		21	21	Feb.	1	20		9	9
	(−2	22)		24	0	T Crucis				5	6		10	6
Feb.	4	9		25	5		(−2	2)		8	15		11	3
	12	7		26	9	Feb.	5	1		12	0		12	0
	20	5		27	14		11	18		15	10		12	22
	28	3		28	19		18	12		18	19		13	19
ζ Geminorum				29	23		25	5		22	5		14	16
	(−5	0)	V Carinæ			R Crucis				25	14		15	13
Feb.	9	19		(−2	4)		(−1	10)		28	23		16	11
	19	23	Feb.	7	8	Feb.	2	18	S Triang. Austr.				17	8
RU Camelop.				14	0		8	14		(−2	2)		18	5
	(−9	12)		20	17		14	10	Feb.	4	7		19	3
Feb.	4	8		27	10		20	6		10	15		20	0
	26	14					26	2						

Maxima of Variable Stars of Short Period not of the Algol Type.
Continued.

RW Draconis		X Sagittarii		β Lyræ		XZ Cygn		SW Cygni	
d	h	d	h	d	h	d	h	d	h
Feb. 20	21	(−2	22)	(−3	2)	Feb. 9	7	(−1	7)
21	18	Feb. 6	22	(−3	7)	10	6	Feb. 3	9
22	16	13	22	Feb. 9	20	11	4	7	5
23	13	20	22	16	7	12	3	11	2
24	10	27	23	22	18	13	1	14	22
25	7	Y Ophiuchi		29	5	13	23	18	18
26	5	(−6	5)	κ Pavonis		14	22	22	14
27	2	Feb. 1	16	(−1	7)	15	20	26	11
27	23	18	19	Feb. 3	2	16	19		
28	20	W Sagittarii		12	5	17	17	η Aquilæ	
29	18	(−3	0)	21	7	18	15	(−2	6)
		Feb. 5	21	U Aquilæ		19	14	Feb. 5	16
RV Scorpii		13	12	(−2	4)	20	12	12	20
(−1	10)	21	2	Feb. 1	20	21	11	20	0
Feb. 6	8	28	16	8	20	22	9	27	4
12	9	Y Sagittarii		15	21	23	7		
18	11	(−2	2)	22	21	24	6	S Sagittæ	
24	12	Feb. 2	23	29	22	25	4	(−3	10)
		8	18	XZ Cygni		26	3	Feb. 6	14
RV Ophiuchi		14	12	Period 11.2h		27	1	15	0
minimum.		20	7	(−0	4)	28	22	23	9
Feb. 4	9	26	1	Feb. 1	20	29	20		
8	2	U Sagittarii		2	19	V Vulpeculæ		X Vulpeculæ	
11	18	(−2	23)	3	17	(−1	3)	(−2	1)
15	11	Feb. 5	0	4	15	Feb. 2	20	Feb. 3	22
19	3	11	18	5	14	10	20	10	6
22	20	18	12	6	12	18	19	16	14
26	12	25	6	7	11	26	19	22	21
				8	9			29	5

GENERAL NOTES.

Different Explanations of the Canals of Mars. In *Harper's Magazine*, December 1907, Professor William H. Pickering gives a brief statement of the different explanations of the Canals of Mars.

First that they are artificial water courses. Objection to this is the extent of them over nearly level surfaces would require gigantic pumping machinery to force the water rapidly so far. Also in so thin an atmosphere water would evaporate rapidly.

Second, if Mars is inhabited and its vegetation is cultivated, Professor Pickering suggests how its shrubbery might have the line-like look we see by the aid of the telescope. This view is illustrated by photographs taken while on a visit at the Azores Islands recently.

Other explanations are the same as those published in this magazine Vol. XII, p. 439; *Science* 1888 vol. XII. p. 82.

No explanation of these singular markings on the surface of Mars is yet given that well satisfies all the conditions.

Request Concerning the Transit of Mercury, Nov. 14, 1907. As far as the unfavorable conditions will permit, the writer desires that certain special physical observations be attempted upon the Planet Mercury at its transit, November 14, 1907. The writer had most interesting experiences in observing the transit of Mercury in 1878, and the transit of Venus in 1882. During these transits he tested a delicate method for successfully ob-

serving the physical appearances of the planets, and he now desires this method, described in Washington observations for 1876, article "Transit of Mercury," to be applied to the determinations of at least one particular point.

In 1878, when the planet Mercury was projected about midway upon the limb of the Sun between first contact and second contact, the writer, with rested eye, glanced into the telescope and was surprised to find that the *entire disk* of Mercury, including that part *off the solar edge* showed in a faint, grayish illumination. The writer has good reasons for believing that this appearance can only be seen at a *critical time* midway between third and fourth contacts on the 14th, and then only seen under good conditions of observation. Adjust accurately to focus, use a rested eye, try the method of glimpsing at image so as to use full sensitivity, and all retinal rods and cones. Swing telescope back and forth so as to move solar-planetary image over *all* retinal rods and cones.

Use these processes *before* and *after* midway position, so as to eliminate "physiological" effect. Especially use this method *at midway position*, because entire disk very likely shows *only then.*

Far best, and latest, observations of fourth contact rapidly move image by swinging telescope. Mercury as *complete disk* MAY thus be seen projected on Chromosphere of Sun.

MONROE B. SNYDER.

Philadelphia Observatory,
Nov. 9, 1907.

If this note had been received in time for the November number of this magazine, doubtless observers of this interesting phenomenon, would have given particular attention to the requests made. As it is, if any observers can give Professor Snyder the information he wishes from the observations of the transit as they were made, the same will be very gratefully received. [Ed.]

Transit of Mercury. Professor M. Moye, of Montpellier, France, writes, November 14, that the transit of Mercury was seen by him in a very fine sky. At ingress and egress no luminous ring was observed outside of the Sun, but the black ligament was conspicuous for fully a minute. He says:

"At no time did I see the faintest suspicion of a halo or ring around Mercury, or any luminous spot on its disk. The planet was black and without any abnormal feature in any form. In short, it was a perfect geometrical transit."

3 rue Achille-Bégé, Montpellier, France.

The Transit of Mercury at Goodsell Observatory.—The Sun was near the horizon and the seeing was very poor, the image boiling badly, but the times of egress of Mercury are probably not more than five or six seconds in error. The observations were made with the 16-inch refractor and and polarizing eyepiece, with magnifying power 225. The times were noted by means of a watch which was compared with the Central Standard Time clock both before and after the observations. The reduced times of the two contacts are:

	h	m	s	
Contact III (interior)	1	48	28 ±	Greenwich mean time.
Contact IV (exterior)	1	50	48 ±	" " "

I could at no time see illumination of the edge of Mercury off the Sun, although at times between the two contacts I could imagine that I saw the whole outline of the black disk.

H. C. WILSON.

Transit of Mercury, Detroit Observatory. Observations of the Transit of Mercury, November 14, were made at Ann Arbor by Professor Hussey, with the 12-inch refractor, and by the writer, with a 4-inch. The atmospheric conditions were very poor, because of the Sun's low altitude.

The table gives the Greenwich Mean Times of contact observed here, and also those published in POPULAR ASTRONOMY for December. It also shows the geocentric times of the American Ephemeris, corrected for the positions of the observers, (which was not done in all of the published times): and the difference between computed and observed times.

It will be seen that the observed times of third contact, (which are naturally more accurate than fourth), range on both sides of American Ephemeris times, showing that the errors are in the observations.

That all observed values of fourth contact are too early, is doubtless due to the fact that Mercury was lost before it was really tangent, because of irregularities in Sun's limb.

Transit of Mercury November 14.

Observer	Place	3rd. Contact			4th Contact		
		Observed	Am. Cph.	Dif.	Observed	Am. Cph.	Dif.
		h m s	h m s	s	h m s	h m s	s
Pickering	Cambridge	1 48 9	1 48 18.8	+ 10	1 50 37	1 50 57.5	+ 20
Upton	Providence	1 48 26	1 48 18.5	— 8	1 50 48	1 50 57.2	+ 9
Doolittle	Philadelphia	1 48 26.8	1 48 17.5	— 9	1 50 39.9	1 50 56.3	+ 16
Hussey	Ann Arbor	1 48 20	1 48 18.4	— 2	1 50 23	1 50 57.3	+ 34
Vinton	Ann Arbor	1 48 20	1 48 18.4	— 2	1 50 35	1 50 57.3	+ 22

WARREN J. VINTON.

Detroit Observatory,
Ann Arbor, Mich.
Dec. 12, 1907.

Group of Red Stars near Nova Velorum. In a cursory examination of Draper Memorial spectrum plates, recently received from Peru, a new gaseous nebula was found, by Mrs. Fleming, on Plate A 8341, taken with the 24-inch Bruce Telescope, on June 6, 1907, with an exposure of 120^m. On further examination this object proved to be the spectrum of Nova Velorum, the new star discovered by Miss H. S. Leavitt, and announced in H. C. 121. The region covered by Plate A 8431, R. A. 10^h 36^m to 11^h 23^m, Dec. — 51°.0 to — 57°.0 (1875) shows so many interesting spectra that a list of them has been prepared, and is given in Table I. All of these are here announced for the first time, unless otherwise stated in the Remarks. The designation and magnitude taken from the Cape Photographic Durchmusterung are given in the first and fourth columns. The class of spectrum is given in the fifth column. Two of these stars have been found to be variable, and one other, suspected of variability, proved to be the known variable, RW Centauri.

REMARKS.

10^h 47^m 35.^{s}2. H. V. 2990. An examination of ten chart plates, taken between June 20, 1900 and May 13, 1905, shows a variation of about 0.9 magn. Estimates from these plates, give the approximate limiting magnitudes, 10.0 to 10.9. A plate taken on April 12, 1898, gives the estimated magnitude 12.0, and increases the amount of variation to about 2.0 magnitudes.

10^h 57^m 15.^{s}9. H. V. 1268. Nova Velorum. Seven bright lines appear in

this spectrum, having the approximate wave-lengths, 5013, 4926, 4862 4643, 4611, 4340, and 4101. Their estimated intensities are 7, 3, 2, 4, 4, 10, and 1, respectively. All of these lines appear to coincide with bright lines in the later spectra of Nova Persei, No. 2, except the fifth, 4611, which perhaps coincides with a bright line at 4608 in the spectrum of H. P. 1311. The strong helium line, 4472, however, which was bright in Nova Persei, No. 2, is absent.

$11^h\ 1^m\ 50^s.8$. The known variable star, RW Centauri. Rediscovered independently in this examination. This star is Z. C. 11^h 129.

$11^h\ 15^m\ 42^s.2$. H. V. 2991. An examination of 11 chart plates, taken between April 12, 1898 and May 13, 1905, shows a variation of about 0.7 magn. Estimates from these plates give the approximate limiting magnitudes, 9.7 to 10.4.

The variable stars, *104055* = H 1275, and 110551 = H 1281, found by Miss Leavitt, and announced in H. C. 122, have spectra of classes K and A 5 F, respectively, on Plate A 8341.

EDWARD C. PICKERING.

Harvard College Observatory. Circular 131.
October 3, 1907.

TABLE I.

RED STARS IN REGION OF NOVA VELORUM.

C. P. D.	R. A. 1875.	Dec. 1875.	Mag.	Spectrum.
	h m s	° ′		
−53° 4086	10 37 2.2	−53 4.1	9.8	Mb5c
−52° 3863	10 38 59.6	−52 26.2	9.2	Mb5c
−52° 3887	10 40 33.6	−52 58.3	8.7	Ma
−54° 4020	10 40 44.3	−54 4.2	9.6	Mb
−52° 3915	10 42 21.6	−52 41.8	10.0	Mb
−55° 3855	10 42 47.7	−55 0.0	10 0	Mb
−55° 3876	10 43 55.2	−55 56.6	9.6	Mb5c
−54° 4080	10 44 42.3	−54 11.8	9.6	Ma
−52° 3964	10 45 59.5	−52 54 8	10.0	Mb
−55° 3919	10 46 25.1	−55 3.5	10.1	Mb
−56° 3935	10 47 1.7	−56 15.6	9.6	Mb
.. ..	10 47 35.2	−52 45.6	..	Na
.. ..	10 49 45.7	−53 26.2	..	Ma
−52° 4048	10 52 0 0	−52 26.7	10.2	Mc
−56° 4069	10 54 11.2	−56 5.3	9.4	Mb
−55° 4046	10 55 1.0	−55 10.5	9.2	Ma
−55° 4075	10 56 50.6	−55 14.4	9.3	Mb
−53° 4299	10 57 11.7	−53 53.6	10.0	Mb
−51° 3819	10 58 40.5	−51 48.0	9.6	Ma5b
.. ..	10 57 15.9	−53 42.8	..	Br. lines
−55° 4114	10 59 39.8	−55 25.2	9 5	Mb
.. ..	11 1 45.3	−53 45.8	..	Mb
−53° 4358	11 1 54.8	−53 11.6	9 4	Mb
R	11 1 50.8	−54 26.8	9.0	Na
−52° 4239	11 2 27.7	−52 7.7	8.9	Ma
−55° 4157	11 3 3.8	−55 19.8	9.8	Mb
−55° 4176	11 4 18 3	−55 20.8	10.0	Mb
−52° 4310	11 6 2.2	−52 49.4	9.4	Ma
−55° 4207	11 6 45.8	−55 32.1	9.6	Mb
−54° 4389	11 7 12.5	−54 17.4	9.6	Ma
−54° 4405	11 8 30.8	−54 10.7	9.8	Ma5b
−53° 4500	11 15 43.0	−53 26.9	9.6	Ma?
.. ..	11 15 42.2	−55 4.8	..	Na
−54° 4516	11 18 39.9	−54 27 1	9.4	Ma5b

The Synodic Period of Jupiter and Saturn. The mean motions in days, from Hill's Tables are,—Jupiter, 4332.5880; Saturn, 10759.2009.

From these the converging portions may be found

$$\frac{5}{2}, \frac{72}{29}, \frac{149}{60}, \frac{2456}{989}, \frac{7517}{6027}, \text{ revolutions}$$

and also the synodic period in years;—

19.85888 or 20 years less 51.5 days.

And the planets are together distant from their starting point. $242°.69 = 242°\ 41'.4$

It is thus seen while the period in time is considerably less than twenty years, the points of conjunction vary but little from the meridians 240° apart.

ROBERDEAU BUCHANAN.

High School Algebra by Slaught and Lennes. This new text-book is published by Allyn and Bacon, Boston, Mass. It is an elementary course in Algebra designed for use in secondary schools. In it the authors treat of number relations fully, by example and precept, of the solution of problems, of simultaneous equations, of special products and factors, of quotients and square roots and of fractions with literal denominators.

The important features which the authors emphasize in this drill book for class work are

1 Algebra is constantly connected with arithmetic.

2 The principles of Algebra are given in eighteen short statements.

3 The purpose of this course is to learn how to solve problems,—not to examine the truths of Algebra for their own sake.

4 The course has been planned with these thoughts in mind in choosing the matter and in shaping its treatment.

It appears to us that this book would do good service in the school room in the hands of a teacher whose ability to teach is in keeping with its spirit and purpose.

Auroral Arches. I was very much interested in reading in your August-September issue, the two references, under General Notes, to "Auroral Arches."

It has been my good fortune while spending my vacations in Canada, to observe some magnificent auroral displays, but the most interesting I think was the one seen by our party at Maple Lake, Ontario, Canada, during the evening of August 8th, 1896. The following notes which I made describe the general appearance:

"About 8 P. M. there appeared in the East a white column or band of smoke which we first believed came from a forest fire. Slowly the column ascended to the zenith and short radial streamers came from a small corona but did not last long—as the white column moved slowly toward the western horizon and gradually disappeared. While the radial streamers were seen in the zenith, rolls of light were also ascending from the northern horizon toward the zenith."

From my notes of the following evening, I find:

"A faint aurora appeared in the northern sky about 9 o'clock; at 10 o'clock the dark segment was quite plainly seen. The light seemed brightest on the upper edge. Streamers appeared at 11 o'clock but there was no recurrence of the east to west band."

This was my first observation of this form of the Aurora and although I

have watched for it each summer during my stay in Canada we were not rewarded until last summer, August 7th, 1906, while we were encamped at the mouth of the Moon River, Georgian Bay and just ten years after the former appearance.

Last year's aurora appeared about 8 o'clock and the band seemed to be about 8° wide, and as in 1896, rose in the east, passed slightly south of the zenith and extended almost to the western horizon.

The luminous matter forming the band or arch was very dense at times although it did not prevent our seeing the brighter stars through it. We could also plainly see a pulsating movement in the band, this movement, as nearly as I could estimate, was about 5° a second.

Between 9 and 10 o'clock beautiful waves or curtains of pale yellow light swept across the northern horizon from east to west, the eastern portion seemed much brighter and higher than the western end.

About 10 o'clock the band or arch became very attenuated and finally disappeared. The streamers from the northern horizon, however, continued some time after the disappearance of the band or arch.

The next evening we again saw the band but it appeared segmental and the luminosity had very much diminished and soon disappeared. We also saw streamers in the north but these were very faint compared with the display of the previous evening.

It is interesting to note the coincidence of the occurrence of the displays of 1871 and 1903 recorded by Mr. Campbell, and those of 1904 and 1907 observed by Mr. Hervey, with the years of Maximum Solar Activity.

If we add thirty-five years, the mean length of the secular period of variation, deduced by Dr. W. S. Lockyer, to the year 1871, we obtain 1906 as the year of recurrence of great solar activity and associated phenomena as aurora, etc.

There is also a close agreement when we add the well-known "mean length" deduced by Dr. Wolf of eleven years to 1896 and obtain 1907 as time of recurrence of activity of 1896 referred to above.

While it is probable that the form of the aurora we have been considering is observed more frequently in higher latitudes where the conditions are more favorable, it does not seem to be of rare occurrence in latitudes south of 45°, and a record of future occurrences will undoubtedly assist in the solution of one of nature's sublime phenomena.

Irwin, Pa. J. WALTER MILES.

Parallax of the Andromeda Nebula.—In A.N 4213 Mr. Karl Bohlin, of Stockholm, gives the results of three determinations of the parallax of the nucleus of the Andromeda Nebula, from measurement of photographic plates. In the measurement of the different plates he states that some 41,000 single pointings were made. The results of the three determinations are respectively +0″.08, +0″.20 and +0″.19, the mean of the three giving +0″.17 as the final result. This places the nebula at only a moderate stellar distance, about 19 light years.

Annular Eclipse.—Under the head of Astronomical Phenomena for 1908, readers will find mention of an annular eclipse in June. Requests have come for a detailed map of the path of this eclipse, and we are glad to say that such a map will appear in the May number.

Errata.—In December, 1907, on page 394, line 6 from the bottom, for "Mitchell's Pass" read *Marshall's Pass*, and same page, line 5 from the bottom, for "18,000 feet" read *10,846 feet*, in J. F. Lanneau's article.

Through an oversight the directorship of Detroit Observatory, Ann Arbor, Michigan, was credited to J. M. Schaeberle in our last number. Professor W. J. Hussey, formerly of Lick Observatory, as most readers of POPULAR ASTRONOMY know is still director at Ann Arbor.